A. Jamme

Miscellanées d'ancient arabe

XIX

In commemoration of
the S I L V E R J U B I L E E of
this series:

I: 1971	VII: 1974	XIII: 1983
II: 1971	VIII: 1979	XIV: 1985
III: 1972	IX: 1979	XV: 1987
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Washington
January 15, 1996

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Foreword

This XIXth volume of my Miscellanées d'ancient arabe is published on January 15, 1996, to commemorate the twenty-fifth anniversary of the publication, last January 15, 1971, of the volume I (82 pp.) of this series. The present volume is composed of 8 sections of greatly unequal length and importance.

The 1st paper (pp.3-36) deals with the analysis of several Sabaean funerary steles to which 4 Safaitic petroglyths are added; all these antiquities show how easily it is for some writers to misrepresent and misdescribe what they are looking at, and in an effort to illustrate some thesis of which the actual origin remains both unknown and unexplained, but also to distort the meaning of the English vocabulary in the simple question of "raiding" and "riding" camel. Is it possible that those authors always remained unable to go beyond the alliteration of the 2 words?

The 2nd and longest section (pp. 37-98) inspects Robin's 2nd panel of his 1992 diptych, viz. Inabbaʾ, Haram, al-Kāfir, Kamna et al-Harāshif, Paris, 2 vol., July 1992, and L'Arabie antique de Karib'îl à Mahomet, Aix-en-Provence, June 1992. It is extraordinary that, at the very start of the 2nd literary panel, the reader is confonted by an obvious discrepancy: instead of "Karib'îl" on the cover = p. 1, he finds "Karib'īl" on p. 3, to return to "Karib'îl" on pp. 5 and 6!? As a long paper on C. Robin had already been planned for this volume, and since the 2 booklets manifest the very same "Robinian" abnormalities, and especially the author's well-known oracular ipsedixitism, his biased argumentations and his pan-classical Arabic fanaticism, a review of either pamphlet would necessarily imply a fastidious repetition of the very same unpleasant falsehoods in this volume. Therefore, the dies were cast, and the choice fell of the 2nd part of the diptych; the review of the 1st part will have to wait for its turn.

The brief 3rd paper (pp. 99-104) is the partial republication of a group of short Thamudic texts, with the hope that good photos of the remaining inscriptions might be at long last available in a not too distant future.

The 4th section (pp. 105-149) is devoted to the pan-classical Ar study of JsaN 17 and JsaT 1 by the J.F. Healey - G.R. Smith tandem. This joint venture was doomed to failure from the start because of the gross superficiality of the epigraphical investigation coupled with the actual rejection - horribile dictu!- of Jaussen - Savignac's invaluable squeezes dating from the beginning of this century.

The 5th section (pp. 150-175) presents Geraldine King's confuse and confusing mishandling of the Thamudic inscriptions of wadis Judayid and Matah. It is quite astounding,

as pointed out below, p.166, that "such a crazy method got her a Ph.D. diploma!"
The 6th section (p. 176) is a minuscule note on F. Bron's erroneous remark on A 692 and YM 362.
The 7th section (pp. 177-230) is the detailed analysis of "C. Robin's misrepresentation of the SA verbal forms," which may best be described as a tendenz novel on the mirages of his grammatical UFOs. Therefore, the discussion of 2 major works of C. Robin takes up almost one half of this volume XIX of JaMAR, viz 114 pages out of 238.
The 8th section (pp. 231-237) is an in-extremis addition devoted to a short review of N. Nebes' booklet on Die Konstruktionen mit /fa-/ im Altüdarabischen in the SA f- pericopes, published by the "Akademie der Wissenschaften und der Literatur - Mainz" in 1995. This brief negative review does not preclude any longer survey of the author's writing.

January 15, 1996

I

The five funerary steles
CIH 419, 445, 723, J. Aylward, RÉS 4575
and the comments of
J. Pirenne, M. Maraqten and M.C.A. Macdonald.

In a lenghty paper devoted to "Wine drinking and wine prohibiting in Arabia before Islam" (cf. PSAS, 23 [1993], pp. 95-115), M. Maraqten demonstrates that nothing comes amiss to him in a paper that is a jumbled meddley inspired by mean journalism mixing up wine with beverages made from honey, barley, wheat, dates, etc, as well as with ethnic Arab groups of different backgrounds. And, so as to force the reader to an easy endorsement of his ideas, he apparently was proud to add the following information: "Acknowledgment: I would like to thank professor W.W. Müller for reading the manuscript of this paper" (p. 111).
Müller's response is irrelevant since Maraqten prefers to remain silent on the contents of that rubber-stamp report. What only matters to the reader is that Müller's recognition necessarily involves his own responsibility: while "reading the manuscript of this paper," he could not have missed the author's testimony which could not be more explicit and more outrageously distorting of the sources. But, as already pointed out in a similar case, "Müller NEVER said that he read them" (cf. my Miscellanées ...,XVIII [1995], p. 89). Maraqten's following double blunder also necessarily casts a long shadow on his credibility and scientific method. As for Müller, a persistent member of A.F. L. Beeston's well-known "laudemus invicem" club, this new example of his performance is well in the line of his profligacy of scattering flatteries all around.

This paper deals first with the study of CIH 419 and 445, on each of which Maraqten wrote one note (p. 98), and also on J. Aylward stela and RÉS 4575 to which Maraqten does not refer.

A - Maraqten's 1st statement is as follows:

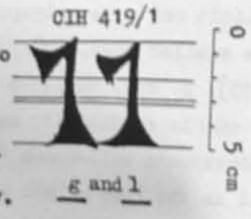
> "Such singing girls were found in the art of South Arabia; a Sabaean woman playing a lute is depicted on a funeral stele (Grohmann 1963: 225, Pl. XVII,1)."

This antiquity that Maraqten fails to identify is BM 125041 = CIH 419, of which a color photo is reproduced by H.St.J.B. Philby, The Queen of Sheba, London, 1981, no. 17 after p. 80. — Far from mentioning the manifest, basic difference between instrumentalist and singer, Maraqten even takes their identity for granted; and he further endorses A. Grohmann's interpretation of the object held by the seated lady (cf. below, p. 6, for my tracing on the left) without referring to C. Clermont-Ganneau's opinion (cf. JA, 15 [1870], p. 306, text reproduced verbatim by CIH, II [1914], p. 102) and al-

so without heeding the photos themselves. Finally, the reading of the authoress' name did not elicit any remark or comment from Maraqten. Therefore, in CIH 419, as well as in CIH 445, Maraqten is interested only, as shown by the title of his paper, in "drinking scenes" (p. 98). His so-called study of the 2 antiquities is garbled and erroneous.

Ever since the earliest presentation of the stela, more than 120 years ago by J. Gildemeister in ZDMG,24 (1870), p. 179, C. Clermont-Ganneau in l.c., pp. 307 and 308 (where J. Gildemeister is named), J. Halévy's copy in JA, 19 (1872),p. 204, no. 680, followed about 9 years later by J.H. Mordtmann in ZDMG, 35 (1881), p. 433, fn 1, until the present day, the 2nd word of CIH 419 has constantly been read "ġllt"(cf. also J. Pirenne, in Syria, 42 [1965], p. 114: "Ghalilat," and G.L. Harding, An Index and Concordance of Pre-Islamic Arabian Names and Inscriptions, Toronto, 1971, p. 457). The 3rd letter, however, is g instead of l, as I have repeatedly claimed against A.F.L. Beeston, J. Ryckmans, C. Robin, etc because of their apparent myopia according to which those 2 letters are very often undistinguishable. The upper left angle of g either encroaches or almost encroaches upon the horizontal line indicating the letter upper limit. Quite the contrary, the same angle of l is markedly below the same horizontal line and, according to the position of the upper angular appendix, l is normally narrower than g. My enlargement of the beginning of the text (cf. tracing on the right) with the letter height of 4.4 cm ebbing to 4 cm shows the 4 following features.

CIH 419/1



g and l

1 - There are 4 horizontally parallel lines finely incised on the letters and defining the lower and upper bands wide of 1.6 (right) and 1.5 cm (left) and 1.7 and 1.5 cm, respectively. A 5th line, 0.25 cm above the 2nd one (from the bottom), starts at the beginning of the text and rejoins the 2nd line at the 2nd t.

2 - The 2 extreme guiding lines are often overlooked by the engraver, viz the tips of many letters encroach upon those lines, such as the top of ġ, t, m, f, etc, and the bottom of t, m, f, etc; contrarily, the top of ṣ of ṣwr (1.1) is 0.2 cm short of reaching the upper line.

3 - The 2nd and 3rd letters of the 1st name are drawn in my tracing (cf. above); they illustrate the respective position of each letter. The top of the left triangle is, respectively, 0.5 and 0.1 cm below the upper guiding line in the 3rd and 2nd letters; and the 3 tips of the vertical section of g encroach upon the 2 extreme guiding lines.

4 - The width of l and g is, respectively, 1.8 and 2.2 cm.

The name of the authoress of CIH 419 is ġlgt Ġulgat. - Cf. Arabic ġuluj "bloom of life." G.L. Harding (cf. above) reads "ġllt" in CIH 419/1, but also in CIH "710/4" (to be corrected to "710/3-4), viz he takes for certain J.H. Mordtmann's reading of "ġ[ll]t"

(cf. Himjarische Inschriften und Altertümer in den Königlichen Museen zu Berlin, Berlin, 1893, pp. 40-41 and pl. 3) with 2 restored letters out of 4, taken from CIH 419 of J. Halévy's copy (cf. above, p. 4) and already corrected by CIH III (1929), p. 130.

B - Maraqten's 2nd assertion reads as follows:

> "Unfortunately, in Arabia we do not have drinking scenes like those of Mesopotamia, Syria or Egypt. Nevertheless, we have a banquet scene from South Arabia; in a Sabaic stele the upper part bears a Sabaic inscription (CIH 445), and the first register shows people drinking wine and listening to music (Grohmann 1963: 227, Pl. XVIII, 1; Müller 1987: 52)."

The following lines complement the remarks of my Miscellanées...,XVIII(1995), pp. 108-109. The lower panel of CIH 445, not mentioned by Maraqten, belongs to the same scene as the upper one, viz the 2 individuals on the right represent ʿglm ʿAglum, the author of the inscription; and both wear the same headgear as the lady on the right of the 2 panels of CIH 419 represents the same woman with the same hairdo, an identity already suggested by C. Clermont-Ganneau (cf. l.c., p. 328), but without any cover, as it is suggested by CIH, II, p. 144: "capite involuto." — Maraqten refers to A. Grohmann, fig. 18, no. 1, where the valueless photo reproduces the incomplete, ugly lithograph published by J.H. Mordtmann in ZDMG, l.c., between pp. 432 and 433. Furthermre, Maraqten apparently did not even notice that Grohmann's plate which he reproduces, does not contain the text existing on the same plate of ZDMG, but perpendicularly set along the right side of the 2 illustrated panels. In addition,Maraqten's reference to W.W. Müller's 1987 paper remains unjustified: Müller does neither present nor mention the wild theory suggested by himself; it contains only a photo of the original; and the best representation still remains the magnificent lithograph of CIH, II (1914), p. 13. Müller's paper also speaks of "Lesung und Übersetzung W.W. Müller" for which I wish to refer the reader to my remarks in my Miscellanées ..., l.c., pp. 107-109.

Below,p.6, are side by side my partial tracings of the main person of each of the 2 steles CIH 419 and 445 so that the reader may judge for himself not only J.H. Mordtmann's interpretation of the central object of CIH 419 as a musical instrument (a lute by A. Grohmann, or a lyre by H. Serig, or a purse by C. Clermont-Ganneau and J.Pirenne) but also of the latter's claim that the main figure of the same represents ... a goddess!

C - J. Pirenne on CIH 419 and 445.

The reader must first know from the start the identity of the so-called parallels used by J. Pirenne to justify her interpretation of CIH 419.

CIH 419
Lady Gulgat hugging her personal handbag on her lap.

CIH 445
ʿAglum's trustworthy valet carrying his master's personal belongings (including cloaks) in a duffel bag.

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a - The photo of the only South-Arabian perfect parallel is not reproduced by her, and its contents are utilized only partially and superficially; some other antiquities are also utlized inasmuch as they serve her purpose.

b - The numerous non-South-Arabian so-called parallels reproduced in her 8 photos and 12 drawings (pp. 122-136) show an eclectic artistic ecumenism as they originate from Afghanistan, Assyria, China, Doura-Europos, Egypt, Gaule, Gaudhâra, India, Kalakh, Palmyre, the Parthe Kingdom, Seleucia, Senjirli and Sudan.
Even at 1st sight, it looks like a pell-mell that may be seen at the stand of a travel agency. Convinced of the veracious imperativeness of her artistic syncretism of forms and designs, J. Pirenne decides quietly, but forcibly that

> "nous allons trouver la déesse ...: assise et flanquée d'adorateurs" (p. 109 on CIH 419) and "nous chercherons la dédicante en bas, mais en haut ...,nous verrons la divinité elle-même" (p. 122 on CIH 419);

and sure enough, she fills up several pages with her ecumenical parallels.

It should be strongly emphasized that Pirenne is really interested only in the 2 following features of CIH 445, viz
a - she refers to the "attribut" (p. 123) of the goddess held by "un enfant" (p. 114) who becomes "le personnage de gauche" (p. 123); and
b - she declares that the upper panel "figure un repas" (p. 123).
That is all: there is no mention, no identification of the seated person of the upper panel or the scene of the lower panel. This is understandable because of the author

being the man ʿAglum. Even Pirenne herself could not feel secure enough and did not dare to speak of a goddess in such a case (cf. also below, p. 13); therefore, she preferred not to reproduce the photo of CIH 445.

D - The images of the figurants of CIH 419 and 445.

The difference in the length of the garments in CIH 419 and 445 stands out clearly. The 1st stela commemorates a woman; the dress of each figurant is long and almost brushes the ankle of each individual; they should be "adoratrices" instead of "adorateurs" in Pirenne's pronunciamento (p. 109). In CIH 445, contrary to CIH's claim of "mulier longa veste induta"(cf. l.c., p. 144), where the text refers to a man, the garment of each person does not go down beyond the calf of the leg and is even shorter in the boy (in the center), where it ends below the knee. In addition, Pirenne's distinction between goddess/upper panel and dedicator/lower panel remains unjustified.

1 - The fundamental objection, viz the presence of a goddess and the god ʿAttar on the same stela,is dodged by Pirenne as follows:

> "En fait, le dieu n'était invoqué que comme vengeur auquel était confiée la garde du monument, selon une formule très courante (nous le savons maintenant)" (p. 112).

The curse involved is well-known indeed, but never in such a case, and nobody may without any hard evidence introduce a female deity in the stela whilst the masculine stellar god ʿAttar is invoked in both texts. Pirenne fails to give a single parallel to prove her case because there is none. It was, thus, safer to drop the matter ... and she never again mentions the name of ʿAttar in the balance of her paper although it is an information important and unquestionable. Instead, she continuously speaks of the goddess the existence of which is only a mirage of her making.

2 - Although always presenting herself as the perfect analyst of beautiful designs, of stylish flourishes and of showy patterns, she fails to mention the striking identities that even at first sight gainsay her distinction between goddess/upper panel and dedicator/lower panel.

On the one hand, the 2 ladies of CIH 419, respectively seated in the upper panel and recumbent in the lower panel, as well as all 3 maidservants have exactly the same hairdo. Similarly, the 2 men of CIH 445, respectively seated in the upper panel and riding a horse in the lower panel, have exactly the same headgear (cf. already above, p.5 for both remarks).

On the other hand, the simplicity of the dress of the woman of the lower panel of CIH 419 is demanded by her condition, as it is abnormal for someone to lie down while fully dressed; and the garment of the 2 men of CIH 445 is identical in form and length, and the vertical appliqués found in different places also are identical.

Therefore, where is the feature so strikingly unique proving unquestionably that the figurant of the upper panel is a deity and the one of the lower panel an ordinary human? All the proof that Pirenne is able to offer is her own personal preconceived mirage based on both her very limited analysis of South-Arabian originals and her superficially material, east-west artistic ecumenism (cf. above, p. 6).

J.H. Mordtmann (cf. CIH, II, p. 103) correctly saw the parallelism between CIH 419 and 445, and his study which he failed to summarize, led him to identify the object as a probable two-string instrument, "vermuthlich ein zweisaitiges Musikinstrument" (cf. ZDMG, l.c., p. 433). The following remarks are in order.

a - Pirenne incorrectly ascribes the interpretation of a musical instrument to CIH. The identification comes from J.H. Mordtmann (cf. immediately above), a fact acknowledged by CIH himself.

b - The identification of the instrument as a lute (cf. A. Grohmann, l.c.,) is disproved by the lack of possible comparison between the object of the stela and an ancient lute; cf., e.g., J.B. Pritchard, The Ancient Near East in Pictures Relating to the Old Testament, Princeton, 1969, nos. 200 and 208, respectively on pp. 63 and 65. The lute has a very long neck ending with a pegbox; on ancient examples shown in the above-quoted references, both neck and pegbox are more than 6 times longer than the resonator itself. Were this proportion applied to CIH 445, the neck and pegbox of the so-called "lute" would be even longer than the width of the upper panel, the upper rim with the text and the central crosspiece with the vine shoots combined. Whatever length these 2 sections might actually have had, absolutely nothing of them appears in both steles; nobody may reasonably claim that they actually existed as nothing of them can be seen on the undamaged original.

c - In the same line of thought comes the interpretation of H. Serig as reported by Pirenne, l.c., p. 123 as follows:

> "M. Seyrig (au vu du relief Aylward) veut bien nous écrire que cet objet lui paraît une lyre (avec traverse supérieure et cordes)."

The photo of the Aylward plaque (cf. A. Grohmann, l.c., pl. 17, no. 1) is not clearly focused enough to allow a good tracing of it. Seyrig apparently made full use of a design in the form of a tau cross above the horizontal wrist (cf. his "traverse supérieure;" cf. also J.B. Pritchard, l.c., nos. 3, 191, 199, etc, respectively on pp. 2,61 and 63), and the 2 connecting groves (cf. his unnumbered "cordes"). In the Safaitic photo published by G.L. Harding's "A Safaitic Drawing and Text" in Levant, 1 (1969), pp. 68-72 and pl. 19, the female performer was also a lyre player (for my remarks on the adjacent texts, cf. my paper on "Safaitic Vogüé 402" in JNES, 32 [1972], p. 18, fn 17), and the form on the lyre is very clear. Seyrig's identification, however, is gainsaid by

the 4 following reasons.

1) - The vertical stem of the tau cross in the Aylward stela is not aligned on the object below the same left wrist, and the extreme right of the horizontal section is ornated with an appendix in the form of an upside-down, isosceles triangle that would be meaningless in a lyre.
2) - The hand of the right arm is completely curled up against the upper chest and grasps something that can obviously not be the upper left corner of a lyre.
3) - The bulkiness of the object does not fit an instrument, such a lyre.
4) - The 2 small circles and the ⋀ made by the 2 converging lines are neither mentioned nor explained (cf. below).

J.B. Pritchard also reproduces a small photo of CIH 445 in l.c., no. 636 on p. 208, with a very succinct description on p. 326A without any mention of a string instrument. There is, unfortunately, the inaccurate mention of "two camels and a rider," as in A. Grohmann, l.c., p. 227.

A. Grohmann describes the Aylward stela as "ein vorgearbeitetes Stück" (p. 226 and pl. 17, no.3); this opinion is also suggested by J. Pirenne (cf. l.c., p.112) because of what she calls the "caractère stéréotypé" of all designs. But, how would a carver make such a stela with all its characteristic designs and ornaments prior to any commission? How could the carver foresee that all those specialized images and embellishments be the choice of a prospective buyer? The stela is another example of an unfinished business, viz the work done to order was canceled before the engraving of the inscription started; cf. the well-known unfinished tomb at Medâ'în-Ṣâliḥ, B 3, a "petite tombe inachevée" in J.A. Jaussen - R. Savignac, Mission archéologique en Arabie (mars-avril 1907), Paris, I, 1909, pp. 327, 487B and photo in fig. 142 on p.329.

d - The identification of the object as a string instrument (cf. J.H. Mordtmann followed by CIH, cf. above, respectively, pp. 5 and 8, and also J.G. Février, La religion des Palmyréniens, Paris, 1971, p. 27: "une femme joue d'un instrument à cordes") is unacceptable.

1) - There is no string in CIH 419. Yet, the object of CIH 419 is the same as in both CIH 445 and the Aylward stela because of its shape and its ornamentation of the 2 holes (CIH 419) = 2 circles (CIH 445 and Aylward).
2) - The so-called strings join together immediately below the left arm in both CIH 445 and the Aylward stela, thus forming a sharp-angled ⋀, which is even more accentuated in the Aylward stela, a conclusive feature that, surprisingly enough, has never been mentioned presumably because a musical instrument with its 2 strings joining together is as odd as a square circle.

3) - As already stated by C. Clermont-Ganneau (cf. l.c., p. 305; reproduced by CIH, l.c., p. 103), "son [= of the seated lady] bras gauche infléchi appuie le dos de sa main sur la cuisse gauche" in CIH 419 (the underlining is mine). Had the place where the dorsum of the hand is resting being an instrument, it is the palm of the hand holding the instrument that would have touched it. Instead, the left hand is supporting the right forearm of which the upward outstretched hand is resting on the chest.

4) - The so-called "strings" of the instrument would be shown in CIH 445 as large, wide groves constrasting with the simple lines on the arms of the youth. Those groves are wider still in the Aylward stela.

5) - The baseless, trapezoidal design engraved on the chest of the same youth (cf. above, p.8), which presumably played some role in H. Seyrig's interpretation, cannot be connected with the object itself because it is not in its longitudinal axis. However, the 2 lateral, convergently aslant lines are concave and joined together near their centrifugally bent upper ends by a horizontal line, aslant to the left. Such features exclude the idea of a string instrument. The whole design seems to be a mere appliqué on the garment itself, which also includes the long, aslant line crossing diagonally the youth's chest and ending in a strongly downward bend; cf. the square design on the chest of the reclining lady in the lower panel of CIH 419 as well as in front of her left leg in the upper panel.

6) - The understanding of the object must also heed the triple fact that the object is connected with the seated lady in both CIH 419 and the Aylward stela, but also with the youth of CIH 445 whose garment is as long as that of his seated master on the right.

J. Pirenne's new interpretation may now be presented. She writes that

> " Il nous a semblé que ce pourrait être une outre en peau d'animal; elle serait censée contenir quelque liquide (eau, huile ou vin) qui symboliserait (avec une convenance toute spéciale pour des Arabes) prospérité et bienfait, figurés ailleurs par la corne d'abondance" (p. 123).

The last 2 1/2 lines belong to her artistic imagery that fills the space without proving anything. Here again, in an effort to boost her theory, she fails to inform the reader that C. Clermont-Ganneau had already written: "le dernier [servant] tient suspendu à son bras droit une sorte de bourse ou the vase (?)" (cf. l.c., p. 306, reproduced verbatim by CIH, l.c., p. 102; cf. already above, p. 3).

But, no human in his right mind would so openly display both the size and the location of his purse. In addition, the origin of Pirenne's interpretation is guided by her artistic, ecumenical journalism that helped her to eliminate the 2 most obvious features of the object for the simple reason that they are outside the field of her syncretic parallels. It normally is very difficult to understand Pirenne's arguments. Here are 3

short examples taken from CIH 419.

How can the reader visualize a large, well-rounded skin container supposedly holding some liquid widely spread out on a seated lady's lap, as would be the case in the upper panel of CIH 419? Who has ever seen or heard of such a scene? Is it not of common knowledge that the waterskin is the first object any traveler gets rid of when halting, by either hanging it on a tree branch or on a peg sticking out of a wall or on the pole of a tent? It would never come to anyone's mind to keep hugging it on his/her lap. In addition, Pirenne's expression "quelque liquide" means some unspecified liquid, as she lists "eau, huile ou vin," and not a small quantity of liquid. This meaning is of course excluded at first sight by the large-sized container either of the ladies of CIH 419 and the Aylward stela or of ʿAglum's valet. But, this is precisely the main stumbling block. A container of the size seen on those 3 antiquities would, of course, never be made to hold only a small quantity of liquid. But, if filled even with water, it would have been much too heavy to be found resting on a lady's lap or hanging down casually from a youth's left arm. Furthermore, since the steles symbolize the beginning of the afterlife, it would be out of place to represent the container less than full ... and no wrinkles indicating that the container is partly filled are visible on the steles. Therefore, it is impossible to think of containers filled with liquid. The preceding argumentation is written with the liquid being understood, obviously enough, as water because even the mere thought of either wine or oil would be preposterous in the present cases.

e - Speaking of the high chair of CIH 419, Pirenne writes this:

> "On remarquera que les pieds en sont terminés en forme de pattes animales (de même que les pieds du lit, au registre inférieur). Nous aurons l'occasion de revenir ultérieurement sur ce détail" (p. 123).

I did not find anything "ultérieurement sur ce détail." But, the lower widenings of the 2 feet of both the chair and the bed legs to the outside are in complete accord, but in reduced proportions, with the serifs of both letters b and ḥ. To see "pattes animales" there is a self-made mirage.

f - On the reclining lady of the lower panel of CIH 419, Pirenne writes this:

> " On n'a pas assez remarqué les gestes de ce personnage couché. Il ne nous semble pas que sa main soutienne sa tête comme on l'a dit; elle nous paraît étendue dans le geste de la prière. Quant à sa main gauche, à laquelle on n'a pas prêté attention, elle est tendue vers le petit cheval qu'elle montre " (p. 124).

Pirenne has just described another of her mirages, as she constantly did in epigraphy, instead of what the original offers anyone to see. The stela shows this:

1) - the palm of the right hand of the lady faces her right cheek, and the thumb rests

upon her right cheekbone while the other fingers touch her temple;
2) - the left hand of the attendant maidservant is resting on both the dorsum of the fingers and the nearby section of the hair of her mistress;
3) - the lady's left arm is fully extended and rests upon her left side; and
4) - the palm of her left hand faces the onlooker, and the thumb occupies its normal position, a ±70° angle with the other fingers which are fully extended in their normal position.

Therefore, it is simply untrue to say that the hand "est tendue vers le petit cheval qu'elle montre." This horse could have represented the lady's favored mount.

However, Pirenne's interpretation gives another example of her systematic misreading of the steles when she writes that, in the upper panel of CIH 419,

> "L'animal dans le champ ne fait pas partie de la figuration conventionnelle puisqu'il manque sur l'exemplaire Aylward. Ce n'est donc pas un animal attribut de la déesse (hypothèse de Clermont Ganneau); ce doit être le rappel d'un don supplémentaire à la déesse" (p. 123).

Firstly, the preceding argumentation is incredible. CIH 419 shows 2 small animals, one on each panel; none exists in CIH 445, which has only 2 large animals in the lower panel; and no animal, small or large, is to be found in the Aylward stela. Therefore, how could Pirenne argue exclusively on ONE small animal of CIH 419 missing in Aylward although both small animals of CIH 419 are missing in both CIH 445 and Aylward? And where is the basis of her imaginative ..."figuration conventionnelle"?

Secondly, Pirenne fails to utilize the same argumenting method on the non-existence of the strings of CIH 419 to eliminate the object as an extra addition to her so-called conventional pattern.

Thirdly, the left forearm of the maidservant on the left and all her fingers are raised vertically and point straight up to a small, short-legged, thickset, tailless and hornless quadruped; the few dots and small cunei studding the animal body (but not its legs) are merely ornamental fillings (as the small cunei on the bird of CIH 723; cf. below, p. 35). This animal understood as a "(lion ou éléphant) à peau tachetée" (!) by C. Clermont-Ganneau (cf. l.c., p. 306; reproduced by CIH, l.c., p. 102; where do such animals live?) is not even mentioned by Pirenne in her description of "Le registre supérieur" (pp. 122-123). That animal is given casually as "le mouton" (p. 125) at the end of her study of "Le registre inférieur" (pp. 123-125), an information that is furtively copied from J. Gildemeister in l.c., p. 178: "ein Schaf (arabischer Race ohne Fettschwang)." That sheep could well have been the lady's pet. The tailless condition of the animal cannot disprove its preceding interpretation as a lambkin because the tail of that animal is rather inconspicuous, and the horse of CIH 445 also remains tailless (cf. below, p. 31). Finally, in spite of the upward direction of both the forearm and hand of the maidservant on the left, Pirenne astonishingly fails to note that the girl is pointing to the small animal above her, which obviously is the case.

It is time to do J. Gildemeister justice. Although he only had at his disposal a lithograph of the upper panel and the 2 lines of the text of CIH 419, his analysis was clear and concise: "Das Bild scheint in keiner Weise religiöse Deutung zu gestatten, sondern lediglich private Darstellung zu sein" (cf. l.c., p. 178).

The presentation of J. Pirenne's case may be concluded by a few remarks.

1 - Pirenne devotes the whole p. 111 to the sole photo of CIH 419, the unique example of her thesis; cf. above, p.6, for the numerous non-South-Arabian antiquities used by her. But, although CIH 445 is vital to her case as the only perfect parallel of CIH 419, it did not interest her very much: she does not even reproduce its photo among the 10 photos of her pl. 8-9 (between pp. 112 and 113). Her lack of interest is readily explained by her inability of using its testimony, viz the male picture of ʿAglum could not be presented even by her as what he should have been according to her preconceived thesis, viz that man should have been a woman (cf. already above, pp.6-7). Therefore, in order to make very sure that the reader would never see in it a reason for gainsaying her thesis, Pirenne preferred, quite logically, not to reproduce the photo of the stela. Thus, the obsession of a preconceived idea leads her to eliminate the perfect parallel, viz a predetermined thesis is the stern rule regulating the acceptability of any parallel! Cf. also below, p.19, as well as pp.234-235, N. Nebes on ʿdbh of Ja 575/11.

2 - The 10 photos of pl. 8-9 with South-Arabian steles, none of which is a perfect parallel of CIH 419, have as common denominator that not a single reference to any of them in Pirenne's text is right, as shown by the following chart:

	Identification of each photo of pl. 8 and 9	
	in the caption	in the text itself
Aylward	pl. 8 a	pl. 9 a, p. 122
CIH 482	8 c	9 c, pp. 118 (twice) and 119
698	8 b	9 b, p. 118
704	9 d	8 c, pp. 118 and 119
708	9 c	8 d, p. 118
723	8 d	9 d, pp. 118 (twice), 119 (twice), 120 and 122
969	9 b	8 a, p. 117
RÉS 4575	8 e	9 e, pp. 117 and 121
Ry 549	9 e	8 e, p. 119
574	9 a	8 b, pp. 118, 120 and 122.

The preceding case is a good example of the irresponsible attitude of the editorial

staff of Syria:without heeding in the least the accompanying manuscript, a wise clerk in
verted the pagination of the 2 plates of illustrations and redistributed the photos of
the original pl.8. Unfortunately, that clerk did neither inform the author of the chan-
ges nor adjust the manuscript accordingly because, most probably,neither was his/her job.
In the meantime, however, the author's every single reference to his own plates was and
remains off. In addition, it remains a mystery how and why the correction of 21 erro-
neous references was not done during the proofreadings. But, were there any? And by whom?

3 - J. Pirenne's preceding presentation of CIH 419, 445, etc illustrates and corrobora-
tes my review of her doctoral thesis entitled Paléographie des inscriptions sud-ara-
bes ..., Brussel , 1956(cf. below pp.44-45), viz that Pirenne has never done anything
but to look superficially at a question and then, to dream a general interpretation of
wave-making boldness and, by using her family name as a shield to splash her boundless
imagination and her very characteristic, theatrical style which facsinated Gonzague Ryck-
mans and 2 Academies and brought them to their knees, spellbound by her cleverness. Thus
when J. Ryckmans and C. Robin printed that my lengthy review in La paléographie sud-ara-
be de J. Pirenne, Washington, 1967, 198 pages, was exaggerated, they confirmed that
which was already a well-known fact,viz they are politicians, not scholars, bent on pro-
moting their own future and on using scientific research for their own purpose (cf. be-
low, p. 45; cf. also J. Ryckmans' political paper in C. Robin, L'Arabie antique de
Karib'îl à Mahomet (cf. above, p. 1), pp. 13-24.

There only remains to deal with the 2nd feature common to CIH 419,445
and Aylward, viz the 2 dots in the 1st = the 2 circles in the latter. They are located
below, and in the perpendicular axis of the lower extremities of the 2 converging
groves, of which the wideness and position suggest them to be straps; they would have
been fastened on the inside of the canvas on the 2 places shown on the outside by the
2 dots = the 2 circles. Two short horizontal slits located at the lower extremities of
the groves and above those 2 dots = 2 circles allow the straps to pass through the can-
vas so as to secure the stability of the bag during its handling by preventing it from
toppling over. When the owner wanted to carry the bag, he had only to tie up the 2 straps
on the neck of the bag, as shown on the stela, to encircle the opening chanel with the
upper ends of the straps and to tie them again ... and the bag was ready to be lifted
and safely carried away.

This bag was the handbag of the lady of CIH 419, but the larger-sized bag carried
by the youth of CIH 445 and the Aylward lady probably was some kind of duffel bag also
transporting larger personal apparel, such as cloaks.

Instead of Pirenne's religious and ecumenical extravaganzas and Maraqten's idyll
of a boisterous dance combined with a drinking bout, for which he had apparently been on

the lookout ever since he decided on the topic of his study (cf. "Unfortunately" of
his quotation above, p.5), CIH 445 contains only the representation of what the family
of the man ʿAglum shown with his feet turned toward the center of the stela, wished to
see on his funerary slab, viz 2 containers presumably of food on the immediate right of
his image, one held in his right hand and the other placed on the right edge of a tall
campstool of the same type as the master's chair, next to a large squarish container on
a tall trapezoidal base, presumably filled with water. On the left side of this contain-
er hangs down a laddle to draw the liquid. Next to the left of the stand, a boy is hold-
ing his left arm in readiness to pick up the laddle, pour some liquid into the tumbler
kept in his right hand and hold it out to his master. Finally, the youth on the extreme
left (cf. above, my tracing of p.6, the right section) apparently is ʿAglum's trustwor-
thy valet with the special duty of attending to his master's personal needs, including
the carrying of his master's duffel bag with his apparel. The feet of the 2 above-men-
tioned male servants are turned to the right, viz toward their master, the ultimate
symbol of their constant readiness to minister to him, as if their feet were saying:"We
are already on our way, Master!"

Contrary to CIH's claim that "Non dubium quin defunctus cibum capiens
exhibeatur" (cf. l.c., p. 144), the 3 basic necessities imposed by eating, drinking and
clothing are met in the upper panel of CIH 445, viz the typical and exclusive needs of
any human being at any time. After death, no ordinary human is in need of a drinking
bout with (or without) an accompanying musician. Maraqten was certainly wishing and look-
ing for details outside the actual contents of the stela, a basically unscientific atti-
tude wholeheartedly approved by the flatterer W.W. Müller.

The lower panel of CIH 445 also badly needs a closer look.

The most important details given by the publisher, J.H. Mordtmann in ZDMG,
35 (1881), p. 433, read as follow:

> "in der erhobenen Rechten eine Lanze schwingend, mit welcher er ein vor ihm her-
> laufendes Kameel antreibt. ...
> Hier scheint der Verstorbene von einem Beutezuge heimkehrend dargestellt zu sein."

But, both the camel and the horse are walking, not running; and neither one word on the
position of the spear in relation with the camel is written, nor one word of justifica-
tion of the word "Beutezug" is to be found.

CIH's summary of Mordtmann's description (cf. l.c., p. 144) reads as follows:

> "videmus eum [= the deceased] eodem vestitu equitantem, dextera hastam vibrantem,
> qua camelum impellit ...
> Defunctus videtur ex expeditione regressus repraesentari."

The word "expeditio" is an ambiguous translation of "Beutezug" that means "raid."
CIH's summary is even shortened by J.G. Février (cf. l.c., p. 27) as follows:

"un homme armé d'une lance et monté sur un cheval pousse devant lui un chameau."

The 3 preceding interpretations have in common 3 unexplained and/or unjustified features. viz ʿAglum "pushes" a camel in front of him; he is returning from an "expedition;" and the location of the lance head with regard to the camel. But, the actual complexity of the preceding positions is clearly distorted. (1) How is it possible to say that ʿAglum "pushes" a camel and at the same time to hide that the spearhead appears embedded far down the bowels of the animal? (2) How is it possible to call such action a "raid" or even an "expedition" or even forget it all together, as Février? (3) Where does the idea of a "raid" come from since the text does not say anything about it?

M.C.A. Macdonald's irresponsible interference.

In a paper published in the 1st issue of AAE, 1 (1990), pp. 24-28, M.C.A. Macdonald unashamedly expectorates the following statements:

> "Representations of raiding [camels] can also be found in more unlikely places ... Thus, on CIH 445 (Fig. 4), the lower relief must surely show the deceased in the act of raiding; his spear-head can clearly be seen touching the camel's side" (pp. 25 and 27, p. 26 is taken by the photo of CIH 445 = fig. 4; the underlining is mine).

My study of Macdonald's worthless paper remains voluntarily incomplete because his shamefully biased artifice is already illustrated by too many examples, and I care not to find even one more.

1 - To list South-Arabia as one of the "more unlikely places" where "raiding [camels] can also be found" shows how seriously Macdonald takes his shortcomings of information. Of course, there was camel raiding in South-Arabia; cf.,f.i., the testimony of B. Thomas, Arabia Felix: Across the Empty Quarter of Arabia, London ,1936,f. i., p. 137.

Section of CIH 445 showing the point of impact and the penetration of the lance, in flagrant contradiction with Macdonald's statements.

2 - The mention of "surely" and "raiding" in his statements already is foolish: not a single word is given to justify either words, and the last sentence, no doubt meant to prove both, actually proves neither. Instead, Macdonald's statements spell out exactly 3 voluntary untruths because the 3 following facts are obviously certain for anyone who is not rooted in deceit (cf. also below, p. 234), viz

a - the lance head and the lower extremity of the shaft (about 1/11 of the total length) look like completely buried in the camel's body and entrails;

b - the point of impact appears to be the croup, not the flank; and

c - such a lancing could not possibly belong to a raid because any animal with such deep a wound would momentarily collapse and die. No sane person would ever think that the purpose of a raid is to collect carcasses.

Macdonald's voluntary distortion of the reality is evidenced by his choice of the verb "to touch" to describe the lance position. The verb "to touch" means "to bring a bodily part into contact with so as to feel, ... to strike or push slightly." In fact, the verb "to touch" is voluntarily erroneous in the 4 cases chosen by Macdonald himself. What an astonishing example: an Oxonian, carefully handpicked and chaperoned by A.F.L. Beeston, unable to find the right word to describe a very simple fact, and after a protracted, careful examination, choosing the word that is contradicted by each of the 4 cases he himself selected ! What a joke! Or is it an Oxonian flash of wit?

- Having autosuggested himself of his pompous verdict, Macdonald utters his triumphant victory, which rings like the rallying yell of the pan-classical Arabic fanatics of A.F.L. Beeston's "laudemus invicem" club, viz the author of CIH 445 is "a man with entirely North Arabian names" (p. 27) with reference to note 9 (p. 28) where he copies the beginning of CIH 445 as follows: "ʿgl bn sʿdlt qryn."

This erroneous transcription is followed by a biased commentary that does not even list a single pre-Islamic Arabic inscription, thus preventing the reader from checking his readings and his statement.

a - When speaking of "a man with ... names" in the plural, Macdonald goes beyond the realm of rationality by considering someone's name as a part, a section of his son's name. What he erroneously copies as "ʿgl bn sʿdlt qryn" is an identity card composed of 3 names, viz. the author's name;
his father's name; and
the author's family name.

b - Here follows the commentary on those 3 Sabaean names, that Macdonald voluntarily swept under the rug to foster his own biased misunderstandings and misconceptions at the expense of the epigraphical facts, but in accord with the basic principle of the pan-classical Arabic fanaticism that distorts everything. This general attitude is all the easiest for Macdonald to adopt because of the following reasons.

(1) He had already vilely copied the author's name erroneously as "ʿgl" instead of ʿglm, thus eliminating the South-Arabian characteristic ending, the mimation -m. This distortion of the author's name fits too well Macdonald's argument to be considered a mere misprint: it is a deliberate tampering with the wording of CIH 445.

(2) He fails to point out that a personal name composed of 2 names rather frequent in South-Arabian is nonexistent in North-Arabian.

The absence of the following onomastical commentary on the 3 Sabaean names of CIH 445 from Macdonald's paper INDISPUTABLY is a VOLUNTARY BETRAYAL of CIH 445 because he could have found many of the following references in RÉS, VIII (1968), G.L. Harding's An Index ...(Toronto, 1971) and A. Avanzini, Glossaire ... 1950-1973 (Florence, 1980, vol. II). Macdonald's scandalous attitude is inexcusable and will accompany him forever.

ʿglm is the name of a Qatabanian group in RÉS 3858/9;

cf. also ʿgln, the name of a Qatabanian group in TS 1355;

ʿglt, the name of a Qatabanian group in Bellerby-Habban 2 p: z + ʿglt, instead of "tʿllt" of G. Ryckmans, in Le Muséon, 62 (1949), p. 109;

ʿgl of Fâwite 1/1, who belongs to the family ġlwn of Sabaean origin; cf. my Miscellanées ..., XVIII (1995), pp. 125-126, 131-132 and 137;

sʿdlt is the Sabaean name of a person in CIH 408, Ja 2352 and 2831 j, and of a family in Ja 2784 L/1-2, and also a Minean name in RÉS 3060/2 and 3377/1. sʿdlt/qryn of CIH 445/1 was at the origin of the fake RÉS 3211.

qryn is a Minaean personal name in Gl 1276/4;

cf. also qrynm, the Sabaean name of a person in CIH 80/1, and of a family in CIH 80/5;

qrn, the name of a Minaean tribe in Gl 1008/2 and 1276/2.

4 - Macdonald also claims that the text contains a curse that

"is very similar to the curses protecting Safaitic inscriptions and drawings in the North" (cf. l.c., p. 28, note 9; my underlining).

The curse is a part of every human culture; it is a rather simple action in itself although the prerequisite may be complex and the specific vocabulary used in the curses is rather limited. But here again, Macdonald fails to describe the specific words used in the Safaitic curses, thus preventing the reader from comparing the wording in both fields. But, worst of all, the reader will of course be thoroughly dumbfounded when told that Macdonald himself reproduces in his tiny paper one Safaitic text with a curse, LP 325 (p. 24, fig. 1) and discusses it and its corresponding LP 403 also ending with a curse in p. 27, fn 5 (for both, cf. immediately below), thus, giving himself the golden opportunity of comparing for the reader their curses with those of Sabaean CIH 419 and 445. Yet, not only he did not make that comparative study, but he even failed to inform the reader that those 2 Safaitic texts contain a curse each!

The schematic comparison of the Sabaean and Safaitic texts involved is printed in the upper part of p. 19. Considering that the curses of the 2 groups have NOTHING IN COMMON, his statement according to which the curse of CIH 419 "is very similar" to the curses protecting Safaitic inscriptions and drawings in the North" (cf. above) can only ori-

Sabaean	Safaitic
hrš in CIH 445	none
qmʿ in CIH 419, 445	none
tbr in CIH 419	WH 1231 contains no curse; and the copy of WH 1124 is as useless as any of H.St.J.B. Philby's scribblings.

Safaitic	Sabaean
dʿy in LP 325	none
hrṣ in LP 325	none
nqʾt in LP 403	none
ʿwr in LP 325, 403	Different contexts.

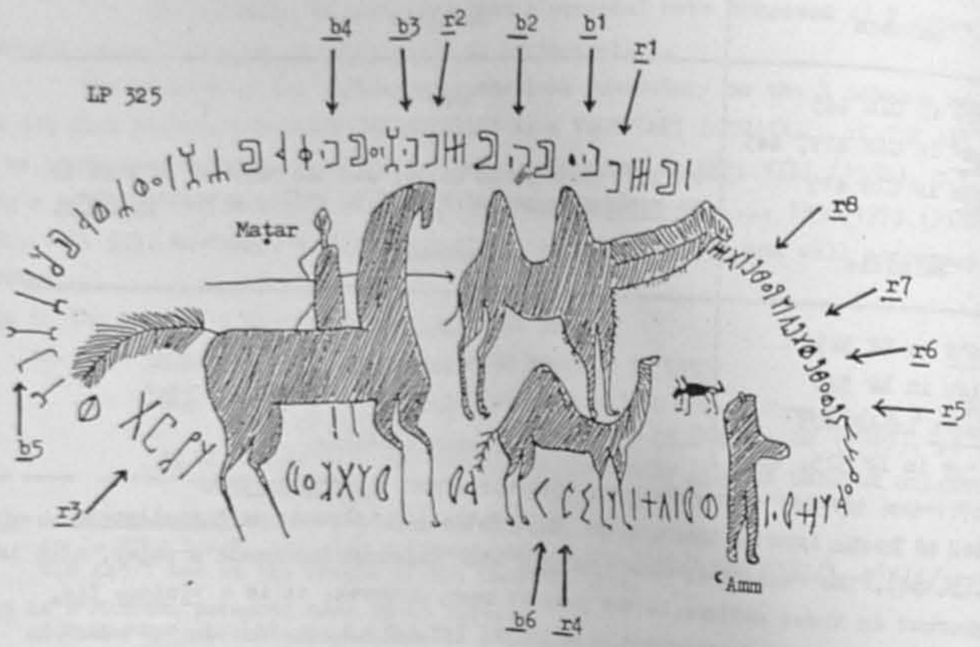
ginate, at best, from an ignorant, and D.T. Potts (cf. already my Miscellanées ..., l.c., p. 54), the "editor-in-chief" of AAE, who welcomed Macdonald's paper, also is an ignorant in these matters. In the present case, however, it is a vicious lie.

5 - Macdonald's 1st Safaitic example is LP 325 (cf. fig.1, p. 24);cf. next page.

a - Macdonald 1st mentions that the facsimile "surely" shows a "camel raiding" (p. 25). Here again, the reader finds the combination of "surely" and "raid" stated most emphatically. But, what is also incredible is that he states exactly 2 lines further down that "a horseman is apparently 'spearing' a camel." He seems thus unable to understand that it is definitely not the purpose of a raider to spear an animal (cf. already above, p. 17), but to catch it healthy and unscathed. If he put "spearing" between quotation marks, it is because he copied it from V.A. Clark, A Study of New Safaitic Inscriptions from Jordan, Ann Arbor, 1983, pp. 394 and 440, respectively, in ClAS 972 and 1192, where the verb is correctly used; thus Macdonald blindly copied someone else's vocabulary without understanding the meaning of it! Macdonald erroneously applied that word to LP 325 to which it manifestly is not and cannot be suited, and he erroneously again substitutes his own solution, "to touch." And, here again, he fails to even tell the reader a valuable information given by E. Littmann who states that

" The two-humped or Bactrian camel is very rare in Arabia and in Syria: so Maṭar may have been very proud of owning such a valuable beast and of having it depicted on the stone "

(cf. Safaïtic Inscriptions, Leyden, 1943, p. 81; in Macdonald's "Sigla" of p. 28, correct "Leiden" to "Leyden"). Thus, Littmann saw that the Asian camel belonged to Maṭar and was not the object of the "raid." Macdonald simply rejects Littmann's opinion without one word and claims to prove his own error by his own ipsedixitism, viz his own



The facsimile shows that the lance head is neither touching the upper camel nor aimed at the lower camel; the horseman, Maṭar, is poised to action by holding his lance for immediate action. - The alphanumerical identifications indicate the location of the 6 b's and the 8 r's of the inscription. - The headless man, ʿAmm, Maṭar's father, is represented as standing at the edge of the desert from which he failed to return. - Left of ʿAmm and facing the small camel stands ʿAmm's dog after returning from his unsuccessful search for his master in the wilderness. For a bactrian, cf. also, e.g., WH 476 and 477.

"surely." As the central section of LP 325 makes it crystal clear, the lance head does NOT EVEN TOUCH the two-humped camel and COULD NOT POSSIBLY BE AIMED at the one-humped camel standing below the other. Yet, Macdonald fails - once more - to even acknowledge the existence of the 2nd camel! Of course, after lying on the upper camel, Macdonald had no problem of lying on the 2nd by sweeping it under the rug ... and that must have been very easy for Macdonald as the 2nd camel should not even be there since the horseman does not "touch" it, as he puts it ... thus threading in the footsteps of J. Pirenne in the case of ʿAglum of CIH 445 (cf. above, pp. 5 and 12), viz directly applying the principle "If an information bothers or opposes you, ignore it" (cf. already above, p. 13).

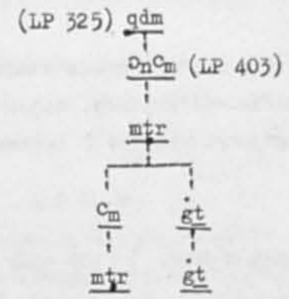
b - Macdonald continues his particular show by failing to point out that none of the animals of LP 325 is even walking: they all are merely S T A N D I N G, which is not exactly characteristic of the action and commotion of a raid!

6 - Macdonald crowns his cairn of errors with his new interpretation (cf. note 5 of p. 27) of LP 325 and 403 (cf. E. Littmann, l.c., pp. 80-81 and 105-106, respectively), both of which had already been coupled together by the editor, E. Littmann (p. 106). The 2 texts read, respectively, as follows:

LP 325: lmṭr bn ʿm bn mṭr bn ʾnʿm bn qdm... wdmy lh ʾbh whrgh mʿ ʾhh mmdbr ...

LP 403: lġt bn ġt bn mṭr bn ʾnʿm wdmy lh ddh ʿm ...

Here follows immediately Littmann's facsimile accompagnied by a few explanatory remarks.



For this genealogical tree, less the top name, cf. E. Littmann, l.c., p. 106. The alphanumerics show the location of the 3 b's and the 2 r's.

Macdonald emphatically states that "w dmy lh ʿbh" of LP 325 "must surely mean 'and his father drew a picture for him."

a - In Macdonald's transcription, his "ʿbh" is an error for ʾbh. This is the 2nd transcription of a very short section of an ancient text and, each time, he succeeds in making one error: a perfect score well worthy of an Oxonian puppet of A.F.L. Beeston! For the 1st example, cf. above, p. 17.

b - The reader is now getting used to Macdonald's "surely;" this is the 3rd time that he throws it at the reader's face like a pinch of pepper aimed at blinding him into submission.

c - Macdonald's strictly "ipsedixitic" approach must be denounced to the utmost once more, viz he makes a pronunciamento ... doomed you are if you don't swallow the pill, because, as C. Robin, he will not give a proof of his verdict.

d - Since Macdonald has his vocabulary confusing such disparate verbs as "to touch" and "to transpierce," it is impossible to even guess at the meaning of an expression,

such as "he drew a picture for," which might refer to either the living beings or the letters, or to both.

Macdonald's following statement meant to indisputably prove that his 1st interpretation is as obvious as any Beeston-like oracle, viz

> "I would suggest a similar interpretation of LP 403, which is drawn by the same artist, this time for his nephew" (cf. l.c.).

What Macdonald had in mind still remains unknown. Fortunately, it does not matter at all: he is dead wrong either way, viz whether he speaks of the inscriptions and/or the pictures.

(1) The inscriptions.

(a) Against Macdonald claiming the unicity of authorship in both cases stands the testimony of the inscriptions that are said to be by 2 different writers, Maṭar and Ġawṯ, respectively; and this duality is proven by palaeography, viz the 2 letters b and r which often are misread one for the other.

For LP 325, cf. above, p. 20.

The normal form of b contains 2 right or almost right angles, ⌋ ; the only exception is b6 with one obtuse angle, ⟨ , and

there are 6 different forms of r,

1 - ⌋ in r 1, 2 (similar to normal b);
2 - ⌊ in r3;
3 - (in r4;
4 - ⌋ in r5,
5 - ⌋ in r6 and 8; and
6 - ⌋ in r7 (same as r4, but rounded instead of angular).

For LP 403, cf. above, p. 21.

There is only one type of b ⟩ which is the same general type as b6 of LP 325; there is only one type of r in r1, the same tracing as r4 of LP 325, but the center of the letter is somewhat less rounded in r2.

(b) The 2nd reason is grammatical, viz the subject of the verb following immediately the identity card normally is the author himself. Such is the rule which is further manifested here by the subject of hrṣ to be on the lookout for being evidently also the author of the text. A change of subject is normally introduced by the conjunction f. The preceding rule was respected by Littmann, but not by Macdonald, who disrupts the normal development of the text by giving dmy a subject different from mṭr whom he resurrects as the subject of the immediately following verb hrṣ! That's a certified Oxonian's logic!

(2) The figures.

The figures of LP 325 and 403 are so different in style and composition that it is impossible to contemplate the unicity of their author. The only common point between them 2 is the hatching aslant to the left inside the tracing of the pictures. However,

the left-aslant hatching is prevalent in the LP texts drawn during the same period, as shown by the following information taken from the LP copies: it is found more frequently than all the other fillings combined in both pictures and large letters, viz

pictures: blank: LP 62, 73, 78, 239, 282, 481, 482, 489, 513;
black: LP 46;
hatching: vertical: ||| : LP 142;
aslant to the left: /// : incomplete: LP 80;
complete: LP 140, 311, 316, 325, 403, 420, 442, 453, 457, 462;

large letters: hatchings to the right: \\\ : LP 195, 197, 311;
to the left: /// : LP 220, 239, 265, 266, 270, 271, 273.

Therefore, the hatching to the left in both LP 325 and 403 is seen more than thrice more often, viz 16 against 5, than all the other fillings combined in the group of LP 160-462, viz LP 140, 220, 239, 265, 266, 270, 271, 273, 311, 316, 325, 403, 420, 442, 453, 457, 462,

and LP 80, 142, 195, 197, 311.

The preceding schema is given although the question must be raised as to how many of those fillings actually belong to the originals. As can be seen in CIS's plates, the fillings in the LP inscriptions are much more numerous than in any other collection, viz that of Dunand; thus, those fillings or, at any rate, most of them, must be presumed to be from Littmann and do not belong to the original petroglyths.

The picture illustrating the vertical center of LP 403-404 (cf. above, p. 21) belongs to LP 403 (but only "probably" according to Littmann, l.c., p. 106, without any reason) because the letters of LP 404 were apparently squeezed and crammed there because of LP 403. In addition, Macdonald refrains from reproducing LP 403-404, therefore, preventing the reader from comparing the pictures.

The mention of lt Lat in both LP 403 and 404 is taken advantage of by Littmann to claim that the female picture "is probably a representation of Allāt" (p. 106) with reference to his booklet on Thamūd und Ṣafā, Leipzig, 1940, "p. 118." In fact, Littmann starts on p. 117 where he introduces Vogüé 402 published by R. Dussaud in Les Arabes en Syrie, Paris, 1907, p. 146: "Déesse safaïtique," and also in La pénétration des Arabes en Syrie avant l'Islam, Paris, 1955, p. 143: "déesse nue ..." (p. 142). Unfortunately, Dussaud's statement on a goddess remains without any justification. For Littmann's commentary in Thamūd ..., p. 118, on LP 240-242, cf. my remarks in JAOS, 91 (1971), p. 139A. CIS (1950) mentions simultaneously 3 opinions on the figure, which would be either a male singer ("cantor") in CIS 1104 (p. 148B), or a naked woman in CIS 464, 1259 and 4351 (pp. 70A, 170 and 542), or the goddess Ruḍā in CIH 286 and 3146 (pp. 46 and 410B). F.V. Winnett, for his part, drastically transforms the whole situation by declaring that "the drawings may be merely an expression of male interest in the female

such as "he drew a picture for," which might refer to either the living beings or the letters, or to both.

Macdonald's following statement meant to indisputably prove that his 1st interpretation is as obvious as any Beeston-like oracle, viz

> "I would suggest a similar interpretation of LP 403, which is drawn by the same artist, this time for his nephew" (cf. l.c.).

What Macdonald had in mind still remains unknown. Fortunately, it does not matter at all: he is dead wrong either way, viz whether he speaks of the inscriptions and/or the pictures.

(1) The inscriptions.

(a) Against Macdonald claiming the unicity of authorship in both cases stands the testimony of the inscriptions that are said to be by 2 different writers, Maṭar and Ġawṯ, respectively; and this duality is proven by palaeography, viz the 2 letters b and r which often are misread one for the other.

For LP 325, cf. above, p. 20.

The normal form of b contains 2 right or almost right angles, ⌋ ; the only exception is b6 with one obtuse angle, ⟨ , and

there are 6 different forms of r,

1 - ⌋ in r 1, 2 (similar to normal b);
2 - ⌈ in r3;
3 - ⌈ in r4;
4 - ⌋ in r5,
5 - ⌋ in r6 and 8; and
6 - ⌋ in r7 (same as r4, but rounded instead of angular).

For LP 403, cf. above, p. 21.

There is only one type of b) which is the same general type as b6 of LP 325; there is only one type of r in r1, the same tracing as r4 of LP 325, but the center of the letter is somewhat less rounded in r2.

(b) The 2nd reason is grammatical, viz the suject of the verb following immediately the identity card normally is the author himself. Such is the rule which is further manifested here by the subject of hrṣ to be on the lookout for being evidently also the author of the text. A change of subject is normally introduced by the conjunction f. The preceding rule was respected by Littmann, but not by Macdonald, who disrupts the normal development of the text by giving dmy a subject different from mṯr whom he resurrects as the subject of the immediately following verb hrṣ! That's a certified Oxonian's logic!

(2) The figures.

The figures of LP 325 and 403 are so different in style and composition that it is impossible to contemplate the unicity of their author. The only common point between them 2 is the hatching aslant to the left inside the tracing of the pictures. However,

the left-aslant hatching is prevalent in the LP texts drawn during the same period, as shown by the following information taken from the LP copies: it is found more frequently than all the other fillings combined in both pictures and large letters, viz

pictures: blank: LP 62, 73, 78, 239, 282, 481, 482, 489, 513;
black: LP 46;
hatching: vertical: ||| : LP 142;
aslant to the left: /// : incomplete: LP 80;
complete: LP 140, 311, 316, 325, 403, 420, 442, 453, 457, 462;
large letters: hatchings to the right: \\\ : LP 195, 197, 311;
to the left: /// : LP 220, 239, 265, 266, 270, 271, 273.

Therefore, the hatching to the left in both LP 325 and 403 is seen more than thrice more often, viz 16 against 5, than all the other fillings combined in the group of LP 160-462, viz LP 140, 220, 239, 265, 266, 270, 271, 273, 311, 316, 325, 403, 420, 442, 453, 457, 462,

and LP 80, 142, 195, 197, 311.

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figure" (cf. F.V. Winnett and W.L. Reed, Ancient Records from North Arabia, Toronto, 1970, p. 75; for my reviews of this work, cf. Orientalia, 40 [1971], pp. 481-489, and JAOS, 92 [1972], pp. 519B-529B). To prove the plausibility of his suggestion on the "male interest in the female figure" which must have been an ever-present aspect of the life of those men, Winnett should have justified not only the extreme scarcity of such designs, but also the absence of all explicit detail in the existing figures. For my discussion of those and similar pictures, cf. my paper entitled "Safaitic Vogüé 402" in JNES, 31 (1972), pp. 16-21, and p. 20 for my reading of Vogüé 402 a+b = CIS 4351+ 4352.-The decisive information is double, viz not a single of all the parallels refers explicitely to the picture of a goddess. Instead, the picture is called hġlmt the young girl in CIS 286, and hqnt the slave-girl in LP 143, each picture representing a female dancer or choreographer, more or less adorned with necklace, arm or leg bangles, and belt. Indeed, why should a well-adorned female dancer be a goddess when the picture is said to represent one of 2 unsophisticated young women described according to their social status? For a Safaitic dancing girl who also was a lyre player, cf. above, p.8.

The last point to be raised in connection with LP 403-404 is that the picture of a dancing girl accompanies an inscription written apparently for the sole purpose of mourning a relative. It is also a fact that the author of the text, Ġawṯ, assuredly knew what he was doing and talking about. Therefore, who can exclude the possibility that the author wanted to inform the reader that he had hired a dancing girl for his uncle's funeral ceremony? Or did he wish to suggest that his uncle's most enjoyable pastime was to attend a fiesta with a dancing girl? Or was it for any other reason known to him and his community and, of course, unknown to us?

Summarily, LP 325 presents a very sad story: the author Maṭar states (1) his identity and tribal affiliation;

(2) his authorship of the picture commemorating his father; and that,

(3) after his unsuccessful lookout for his father (hrġh) from the desert in the company of his cousin (mᶜ ʾnh) Ġawṯ, and even of his father's own dog, he had to face the fact that ᶜAmm, his own father, had not survived his trip into the desert.

Maṭar also wishes to inform the reader that he had been at the ready with his lance during the lookout, well prepared for any eventuality, and also with his 2 favored camels-in-waiting ready to carry home his father and his belongings if he was lucky enough to emerge from the wilderness. Unfortunately, it was all in vain, and Maṭar manifests his sorrow in a large petroglyth dedicated to his close relative who did not return from the desert. E.Littmann (cf. l.c., p. 81) had already referred clearly to "Maṭar in horse-back with a two-humped camel, a dromedary and a dog, starting to look out for his father." But, there is also a 2nd man, a headless one, with his back turned to the dromedary, and Littmann notes that "It may be that the indistinct figure denotes the father who appeared in a vision." But the figure is headless, not "indistinct," and where is the proof of "a vision" as dmy to draw does not presuppose "a vision"? Also, why would Maṭar leave the drawing of that man incomplete? And, where is the proof that Maṭar failed to finish his drawing? There is no indication that he indeed did so. Therefore, it would be unfair to reproach him of having done what he wanted to do. Indeed, Maṭar's drawing might suggest that ᶜAmm had been suffering from a mental state depriving him of the normal use of his mind. If such is the case, he acted like a headless man, and this would explain his uncalled-for trip into the wilderness, and both Maṭar's and his cousin's subsequent expedition to the fringe of the desert in a fruitless effort to bring him back home.

Two last details of the petroglyth are worth considering.

a - Littmann correctly mentions the presence of a dog without any further remark. Yet, the position and the location of that small animal are intriguing, viz the 3 big animals and the 2 men are all facing the right, but the dog fronting toward the main group on the left is standing between the lower camel and the headless man whose feet also are turned to the right. The fidelity of a dog to its master is legendary. The dog of the drawing may be that of the deceased that Maṭar would have taken along as a precious help in the search. The dog is shown back from the trail of its master's scent that it had followed for awhile, but it was in vain as indicated by its position of standing behind its master turned in the opposite direction.

b - A special consideration should be given to the posture of the 4 animals. Although they all are standing, the rearing horse with its tail outstretched seems to be expecting something. In a much calmer mood is the Asian camel with the neck outstretched which seems to be looking intently ahead. The dromedary, however, has straightened out its neck almost vertically holding up its head as if surprised at hearing or seeing something. And, strangely enough, it is the little dog that is facing the dromedary.

LP 403 is easy to understand: Ġawṯ, the cousin of Maṭar, the author of LP 325, also pays tribute to the same ᶜAmm, his uncle (ᶜmh) and father of Maṭar, because of his painful disappointment after beeing with his cousin on the lookout for their common relative who had vanished in the waste land.

7 - Contrary to LP 325, CIAS 972 and 1190 (cf. V.A. Clark, l.c., pp. 394, 576 and 440, 589, respectively), both reproduced by Macdonald on p. 25, and also below, p. 26, offer 2 images of camel hunting because in either cases both the lance head and the end of the lance have penetrated into the camel's body, either in the anal area (CIAS 972) or in the disproportionately bulgy hump (CIAS 1190). Such wounds make recovery unthink-

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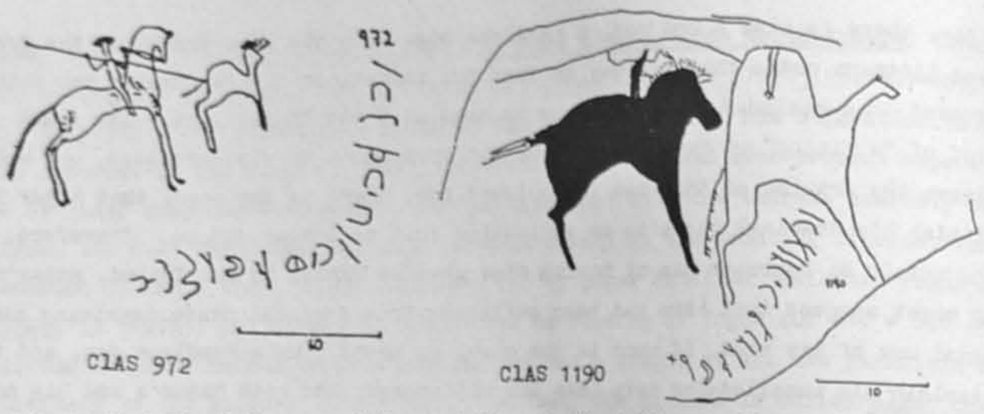


able. Note that the 2 drawers did not delineate the hidden sections of the weapons.

Those drawings failed to elicit any remark from journalistic Macdonald. In CIAS 972, the editor speaks only of "a young camel." The 1st objection is that a professional hunter would hardly take pride in killing a baby camel. Secondly, the animal is indeed much smaller than the horse in the drawing. However, Safaitic drawers never in the least attempt to respect the physical proportions of either the form or the size of any section of a picture, viz the man and the horse are manifestly disproportionate, but they are still more proportionate than the extravagantly arched size of the hump and the fantastic bend of the lance of CIAS 1190. Therefore, nobody should hastily evaluate the actual size of any part of a picture on the size seen in the drawing, viz in the present case, the size of the camel does not suggest it to be "a young camel." Since the author Bard apparently wrote his short inscription to introduce hfrs this [= his own] horse, he must have been very proud of it, to say the least, and he made full use of the drawing to boost his pride by reducing the size of the hunted camel and by the same token, by increasing the size - and, therefore, the strength and the performance - of his own pride horse. Clark refers to the picture of CIAS 1190 as dmyt without a single word on the ornamentation visible on the abnormally bulgy hump: it looks like a long sash thrown across the hump and hanging down on the camel's side with a triple tassel at its extremity. If it is what it looks, the camel could not be wild; it was a domesticated camel that had gone away because it had gone berserk or had been stolen. Apparently, it could not be recaptured harmlessly and had to be destroyed. If such is the case, that would support the 1st suggestion, viz the camel was berserk and, therefore, had become unusually dangerous for men and beasts alike. Cf. below, p. 36,*.

A final note on the 2 preceding petroglyths.

There is no photo available of CLAS 972. In the copy of the thesis received from Melbourne, photo B of pl. 10 does not at all allow the reader to check Clark's facsimile: almost nothing is distinguishable. Furthermore, the 30 photos of the 10 plates of the

Ann Arbor "University Microfilms International" publication are a perfectly useless waste of money coerced by an organization of which the aim is to financially profit from scientific publications under the pretence - indeed admirable in itself - of spreading scientific studies and helping scholars to get the material information although, in fact, not caring whether an essential part of any particular publication is useful or dead loss for the buyer who, therefore, is charged for many worthless pages.

8 - In note 1 (p. 27), Macdonald speaks of E. Anati, Rock-Art in Central Arabia, Louvain, II, 1968, "59, Pls. 4 and 6 ... where spears which have been thrown are shown sticking into the camel."

Pl. 4 is on p. 57, not "59." - Anati speaks of this plate in p. 54 with a facsimile of the lower half in fig. 4: "All four animals are wounded by one spear or arrow." But, the so-called weapons have each about the same length, and Anati fails to say which one is, in his opinion, a "spear or arrow." In addition, what is quite amazing is that all 4 weapons penetrate the BELLY of the animal ... FROM BELOW!, viz

one of the weapons is almost vertical (in the 2nd animal from the left);
two are slightly aslant (in the 2 extreme beasts); and
the remaining one is at a 45° angle.

Yet, such features seem to have hypnothized and mystified everybody because no question has ever been asked, and everybody seems to remain with his mouth gaping open as if gobbling the surrealistic exploit. But, how could a hunter achieve such a feat unless the beast lies down, which is not the case since all animals are standing? And, how can anyone call any of those weapons a "spear or arrow"? Each one is too short to be a "spear" unless it be a Pygmy's "spear," and too long and too thick to be an "arrow." Finally, if the 4 animals were actually wounded as shown in the picture, the attack by 2 hunters at close range would be both useless and uncalled for as the doomed animals would collapse and die almost momentarily ... and when the animal is down - but such is not the case in fig. 4 - a hunter may step in for the kill, but this cannot be called an attack. The 2 hunters' attacks must be some kind of bragging or a mere mockery.

Other cases of animals with lances embedded in their belly from below also come from Saudi Arabia; cf. Atlal, 5 (1401/1981), pl. 34C-E and 36A, examples given by Macdonald to illustrate camel hunting. Such representations, however, cannot depict actual hunting scenes; they must be symbolistic. Not only, as already noted above, no hunter can do such a deed, but also the drawings show the exact location and penetration of both the extremity of the lance and the lance head inside the animal's body; and this is impossible since those parts cannot possibly be seen.

Note on E. Anati's description of his fig. 4 (p. 54).

Speaking of the hunters, the author writes that "Both men hold a dagger at their belt and use a strange curved stick for hunting the animals." The last weapon is also seen in the hand of 2 hunters in Anati's fig. 5 (p. 55) or pl. 5 (p. 57) where he speaks of

> "a bent object which at first seemed to illustrate a sickle-sword. A consideration of several other such figures from Arabia led us, however, to the conclusion that it should represent a boomerang or some other sort of throwing-stick."

Not only Anati refrains from giving the references of those "several other such figures," thus preventing the reader from checking them, but his interpretation also is unacceptable in fig. 4, where the hunters face the animals and stab them with those weapons. For the curve of those weapons, cf. that of the spear of CIAS 1190 (cf. above, p. 26).

Finally, Macdonald adds that "the grounds for dating these drawings to the third millenium BC seem to me somewhat tenuous, though they clearly pre-date those attached to inscriptions" (p. 27, note 1). Here again, he fails to justify his opinion (for other similar cases, cf. already above, pp. 16, 17, 18 and 19) or to acknowledge that I was the very first to reject E.Anati's views a quarter of a century ago in my Miscellanées ..., 1 (1970), p. 77-81; that my rebuttal was justified by the analysis of the photos used by Anati; and that many petroglyths actually are contemporary of some inscriptions; cf. l.c., p. 74:

> "The color photographs now at my disposal, e.g., from ʿAn al-Jamal (no. 5, pp. 22 - 25) and an-Naḍîm (no. 6, pp. 25 - 26), positively contradict the author's classification because many inscriptions have the same patina as that of many figures of animals and men around them."

Needless to say that the expression "around them" refers to texts connected or not to the petroglyths and that I have seen, photographed and copied all of them.

9 - In note 2 (p. 27), Macdonald telescopes his sources when he claims that E.A. Knauf bases his dating of 3000 B.C. till 100-200 A.D. for the hunting of wild camel in Arabia upon A.R. al-Ansari's Qaryat al-Faw (cf. also pp. 24-25). Here again, Macdonald fails to acknowledge that Ansary does not give any date at all (cf. pp. 26B and 130-133); and that Knauf explicitely refers to E. Anati for the date of 3000 B.C. (cf. l.c., p. 75: "should be dated in the middle of the third millenium B.C.)." And those Ansary's illustrations, according to Macdonald, "must surely represent raiding rather than 'camel hunting' as suggested by the excavator" with reference to note 10 (p. 28) where the same pagination is given, viz "130-133."

The reader is well accustomed by now to Macdonald's aria of "surely." Here again, the camel of pp. 130-131 (also reproduced in a small rectangular inset in p. 132) seems to be stunned and to slightly drop back upon the impact of the weapon just at the base of its throat. The hunter on horseback with the name of "Sâlim, son of Kâʿib" (cf. already read by Ansary, l.c., pp. 26B and 131) above him is behind the camel and further lances the animal in the croup in a 2-prong attack. The camel of pp. 132-133 (also reproduced in a small rectangular inset in p. 133) was walking when the long lance of the horseman called "Mâlik" (also read by Ansary, pp. 27B and 133) struck it in the middle of the hump.

It seems incredible to me that, considering the wounds inflicted to those 2 camels, Macdonald would still maintain his theory that such scenes "must surely represent raiding rather than 'camel hunting'" (cf. already above, p. 19). But, as "to touch" can actually mean also "to penetrate deeply" in Macdonald's idiom, it is possible that "to raid" may also mean "to hunt," although his text sometimes suggests the normal meaning of either. Finally, it should be pointed out that Macdonald never refers once to my lengthy, detailed rebuff of the works of E. Anati, V.A. Clark (cf. my Miscellanées ..., XIII [1983], pp. 2-113), E.A. Knauf, E. Littmann, F.V. Winnett, his own papers (cf., e.g., my Miscellanées ...., XV [1987], pp. 192-202) and of W.W. Müller (cf., e.g., l.c., XIV [1985], pp. 132-175).

10 - This last section on Macdonald's paper contains some redactional remarks on his notes 1-6 of p. 27, which show how Macdonald reacts when reproducing what he reads. The preceding pages already contain many examples of his failure to read what is printed, although some of his mistakes may well be voluntary distortions of the facts in order to better suit his thesis or preconceived ideas.

a - For my discussion of his notes 1 and 2, cf. above, pp. 27-29, to which the following should be added. In note 2, l. 7, "London: Croom Helm" must be corrected to "London: Croom Helm Limited," which is erroneous because it is the address of the distributor; the correct name is "University of Riyadh." In addition, "144, no. 4" must be added to "130-133."

b - In note 3, J.L. Burckhardt's Notes ... was printed in 1830, not in "1831;" and Macdonald's quotation comes from p. 84, not "140."

c - In note 6, V.A. Clark's A Study ... was printed in Ann Arbor in 1983, not in "1979" (also given in p. 28), which is the date of the defense of the thesis (cf. below). The Ann Arbor printing is all confused: page A lists "facsimile printed ... in 1983" although the label on the front cover mentions "CLARK, V.A. 1980" (cf. also p. D: "Ph.D. 1980"), and in p. 1, the title bears "University of Melbourne 1979," and the author's "Preface" bears the date of "25th October 1979."

More than thirteen pages of this section (pp. 16-29) are devoted to M.C. A. Macdonald's loathsome paper of AAE. The printing of so repulsive an article is an outstanding, irrefutable example of the kind of intellectual submissiveness and degra-

dation which may be imposed on persons as credentials to be paid for their admission into some restrictive, strongly guarded clubs. It is, however, unquestionable that it takes a special breed of sheepish mentality to voluntarily submit to such "Fourches Caudines," unless it be a political trick which would be even worse. Without such a base mentality and the lustful pruritus for such so-called promotions, those reviling practices would not survive and stand; here also, it is a simple question of "supply and demand." Such a disgusting paper is further welcome because it fittingly warns the reader off worthless papers of the same author, such "Safaitic Inscriptions" in The Anchor Bible Dictionary, vol. 3 (1992), pp. 416A-423A, which practically is a systematic travesty of the Safaitic Material, in the true spirit of F.V. Winnett and G.L. Harding.

The "Conclusion" of my paper of Miscellanées ..., XV (1987), p. 203 (cf. above, p. 29) fits here and indicates to the reader that Macdonald has not changed in the last 8 years and did well earn his recently acquired Oxford key and medaillon, viz

"Conclusion.

Macdonald's whole argumentatior is squarely rejected as lacking good common sense, the basic honesty toward the texts and those who engraved them, and the elementary knowledge of Saf epigraphy. His general attitude very closely copies Winnett's literary production. Both authors have in common a passionate cult for preconcieved ideas and theories which prevent them from looking at the facts without distorting them and which impel them not to really pay attention to anything, texts or persons, that stands in the way of the ideas or theories of their choice."

More on the lower panel of CIH 445.

After reviewing Macdonald's production and his numerous aberrations, even on a lexicographic viewpoint, it is necessary to go back to CIH 445 which Macdonald badly mistreated and in particular to the study of the 3 components of the lower panel of that Sabaean antiquity.

-1- Characteristic of the horseman ʿAglum's position is that, although his left forearm rests horizontally on his lap, his 5 fingers are without any apparent reason angled upward vertically to form some kind of hooked beak (cf. above, photo in p. 16). The left hand of ʿAglum's upper panel is also given the same general twist. That particular position of the fingers may be seen as one of the carver's stylistic characteristics (cf. below, p.33).

-2- The 2 animals, a horse and a dromedary, have both their neck drawn back and their head erected (cf. photo above, p. 16); this double characteristic is also found in CIH 698. The purpose of such a feature is to save space because, had the carver

wanted to represent the 2 heads in their normal position, he would have been forced to considerably reduce the animals' size, and that in turn would have compelled him to equally abate the stature of ʿAglum who might not have appreciated it very much.

-3- The horse's braided mane hangs down as a collar; the neck is festooned with 4 contiguous horizontal bands of which the 2 extremes show some kind of a broken-line decoration; both the noseband and the cheek pieces are rather wide, and the shoulder is also decorated with an angular double line containing a series of large ^ in a long, broken line, ^^^, similar to the upper decorative motive below the head; for the preceding features, cf. also the photo of p. 16. The horse is as tailless as the sheep of CIH 419 (cf. above, p. 13): nothing of the dock is to be seen; the haunch takes almost the left 2/3 of the narrow vertical rim, and the hock of the left leg brushes that vertical rim. Finally, no trace of saddle is detected on the horse.

-4- ʿAglum's head and trunk are represented almost identically in both panels. In the lower panel, however, the trunk is askew to the right side,but seen frontally in the upper section. Therefore, the rounded protuberance below the left elbow,very much in evidence in both panels, represents only the buttocks, and not a cushion or a saddle of which no sign is visible. The same bulge also visible in the upper panel of CIH 419 apparently is the carver's way of representing a person, whether seated on a high chair, as in CIH 419, or riding a horse, as in CIH 445. That protuberance is not found in CIH 698. In addition, as the author is riding a horse, his left leg was moved forward to be at the center of the inferior hem of the garment, compared with the figure of the upper panel (cf. above, the photo of p. 16), viz perpendicularly from the center of the in-between of the legs in the upper panel.

-5- No explanation or proof at all has ever been presented to foster the interpretation according to which the horseman "pushes" the dromedary in front of him. The crucial question, which has never been raised in the past, is the position of the lance and the dromedary; cf. above, p. 16 for the photo of that section of CIH 445. The section of a weapon inside the body of the hunted animal is hidden and, thus, is not shown in other antiquities ((cf, f.i., J.B. Pritchard, l.c., nos. 184, 190 and 319, respectively, on pp. 57, 60 and 104; cf. also above, p. 26 on CIAS 972 and 1190, and p. 27)), but the ropes securing the neck, remaining outside the body, are shown (cf., f.i., l.c., no. 186 on p. 58). In some Saudi Arabian petroglyths already listed (cf. above, pp. 27-28), however,the parts of the lance inside the animal's body are fully delineated, an unjustifiable design. In the present case, the section of the lance above and outside the dromedary is well traced in bas-relief as a long piece of wood of which the upper end encroaches very deeply upon the wine shoot of the central crosspiece. That tracing stops

above the end of the completely curved-up tail of the camel. Even the carver himself makes it clear that he was not drawing the real lance on the dromedary because of the 2 following reasons, (a) the outline of the lower extremity of the lance leaves intact both the tail of the animal and the thickness of the bas-relief on the croup; on the contary, the upper part of the lance outside the animal is in bas-relief; and

(b) the width of the lance outside the animal is 5 mm, while that of the outline on the animal is 3 mm.

The photo now at my disposal, a print of a black-and-while "cliché des Musées Nationaux" labeled "67 DN 1852" shows 2 superposed pairs of tiny, vertical lines immediately below the lance head, which could be understood as drops of blood. Those lines, however, do not exist in CIH's lithograph.

The part of the lance shown on the hindquarters of the dromedary, including its intestines, is superficially outlined on the stela. As already pointed out above (pp. 16–17 and 27), how could a carver trace exactly the trajectory of a lance inside the body of the hunted animal? He simply could not do it and, therefore, if he does, its drawing cannot prove what he drew; it could only manifest what the drawer's imagination suggested him. Therefore, the tracing of the end of the lance on the dromedary's side cannot represent an actual lancing but is limited to suggesting only what part of the lance is outside and aslant to the right side of the animal's hindquarters.

The lower panel of the stela shows a familiar scene of ʿAglum's daily life, viz ʿAglum is riding his favored horse, brandishing his ordinary weapon as the proof of his constant alertness to any possible danger, and is preceded by his dependable companion, a pack dromedary.

Finally, a fundamental error made by Macdonald and others is to equally value and interpret a petroglyth traced on a mountain side or on an isolated slab of the desert as any bas-relief of a stela. The 1st normally refers to an unusually important, even an exceptional event in the life of the author; it is the individual's commemoration of one highlight of his adult life. The purpose of the scene of a stela, on the contrary, is intended to symbolize the whole life of the deceased, not to refer to any particular event; and nobody had camel hunting as a way of life, and especially in southern Arabia.

The analysis of both CIH 419 and 445 shows 13 contact points justifying my contention that both antiquities were made by the very same carver, viz

1 - the main sections of each stela are 2 horizontal panels separated from each other by a narrow median crosspiece;
2 - each stela has a text composed of 2 lines each;
3 - each text begins with the noun ṣwr image;

4 - the curse of each text involves the stellar god ʿAttar, and contains the same verb in 2 almost identical expressions, viz wlqmʿn (CIH 445) = wlyqmʿn (CIH 419);
5 - the 2 texts exhibit the same palaeography;
6 - the lower rim is blank, viz perfectly free of any kind of design;
7 - both lateral rims are very narrow: their proportion with the width of the antiquity is almost identical, viz 1/30.6 in CIH 419, and 1/31 in CIH 445;
8 - vertical lines are used as ornaments on human garments;
9 - the very prominent buttocks of the individual, whether there is a chair or not;
10 - the very long ellipsis representing the right eye in the middle of the right cheek, cf. the left girl of the upper panel of CIH 419, and the central boy of the upper panel of CIH 445;
11 - the vertically beek-shaped hand in
CIH 419, the left hand of the right girl of the upper panel, and the right hand of the right girl of the 2 panels;
CIH 445, the left hand of ʿAglum of the lower panel;
12 - the unusually concave position of the fingers in
CIH 419, both hands of ġlgt of the upper panel;
CIH 445, upper panel, the left hand of both ʿAglum and the central boy, the right hand of the right boy;
13 - in CIH 445, all fingers are pointing upward even when the forearm is in the horizontal position and the hands are not holding anything;
in CIH 419, the preceding remark is also valid with the exception of the left hand of the reclining lady of the lower panel.

The last 5 features (cf. already above, p. 30) may be considered as the carver's real signature.

RÉS 4575 = BM 125341 and J. Pirenne.

An additional note on RÉS 4575 is suggested by Pirenne's use of that antiquity in her paper. She mentions it in passing on p. 115, but gives some details on p. 117, including this:

"dont nous publions ici la représentation inédite (pl. IX, e)" (ll. 24-25) =
"dont nous donnons ici une photographie inédite (pl. IX, e)"

on p. 121, ll. 9–10, without any cross-reference, but with more information and a short study of the piece.

The hammered-out human representation is the problem of the stela. If Pirenne is quite correct in excluding "les deux personnages latéraux" present in the upper panel of CIH 419, 445 and Aylward – there is no place for any of them in the narrow hammered space –, her statement to the effect that "il ne pouvait s'agir de la figure assise

qui serait beaucoup plus volumineuse au niveau des genoux et des pieds" (p. 121) must be repudiated based, as it is, on her assumption that the representation has to be, as in CIH 419, her infallible pattern, that of a woman. Consequently, Pirenne claims that "la silhouette qui se dégage des traces de martelage, ... correspond à celle d'une semblable deesse debout, la main droite levée" (pp. 121-122) of "un relief parthe" (!?!?) of Berlin Museum illustrated by the photo of her fig.2 of the same p. 122. Here again is the antiquated, empty tune of a goddess lifting her right hand, and all the more absurd that "la main droite" of lady Ḫulgat of CIH 419 is not at all "levée," but simply resting on her chest (cf. already above, tracing of p. 6).

Firstly, the only name engraved on the upper rim of RÉS 4575 is rbʿt, that is known in Sabaean (cf., f.i., my Sabaean Inscriptions from Mahram Bilqîs (Mârib), Baltimore, 1962, p. 411A) and in Ḥaḍramawt (f.i., Ja 945/1) exclusively as a masculine name.

Secondly, the 2 steles are not alike from a structural viewpoint: the representation of the Berlin antiquity fills up the whole field; in RÉS 4575, it only fills about half of the width. The right arm is bent upward, but whether the person was holding an object in the hand or had the palm extended, is impossible to determine without examining the original. Three important features, however, are noticeable:

a - the human trunk is slender as the length of the right forearm is clearly shown on the left;

b - the right angles of the shoulders seem clear; and

c - the right side of the bas-relief is slightly to the right of the perpendicular line drawn from the angle of the left shoulder; this detail suggests a narrow vertical back of a chair of some kind.

CIH 723 = Musée du Cinquantenaire (Bruxelles) 0.717.

To the preceding group of CIH 419 and 445, I would like to add CIH 723 as another stela made by the very same carver.

J. Pirenne (p. 118) erroneously states that the 2 persons of Ry 574 "correspondent exactement à ceux que l'on voit sur CIH 723" = "CIH 723 ... les personnages sont identiques à ceux de Ry 574." This statement is made up by Pirenne, as shown by the following chart:

	Ry 574	CIH 723
the main person's right hand	is holding nothing;	is holding a small bird;
his left hand	is holding a long lance;	is holding nothing;
the lower half of the garment of the boy on the right	is ornamented with aslant lines more or less parallel to the lateral edges;	is ornamented in the same way and also with 4 horizontal lines dividing the space in a checkered design.

Secondly, always under the influence of the theory of an offering somewhere, Pirenne speaks in CIH 723 and Ry 574 as follows:

> "Les deux personnages montent un escalier, figuré par la marche sur laquelle est situé le maître-dédicant; ils montent vers le sanctuaire et c'est celui-ci que doit symboliser l'architecture, agrémentée de symboles divins" (p. 120).

But, instead of "architecture" and/or "symboles divins" in CIH 723, there are just 4 plain, bare rims, except for the inscription on the upper rim. And the so-called "step"= "marche" leads to nowhere as it stands alone above the lower rim clearly delineated on both sides; therefore, to claim that "ils montent" including the young man is a shameful contrivance. It is a pedestal used by the master to stand higher than his servant so as to enhance his stature compared with that, already smaller, of the latter. On my photo of the museum, "no. 1255 A," the height of both master and servant is 8 and 6.7 cm, respectively; the servant's height was about 5/6 that of his master.

Both forearms of the servant are bent upward, the left slightly and the right at a 45° angle; the left hand does not hold anything. According to Pirenne, "le petit personnage" is holding "quelque chose d'indéfinissable" (p. 119). It is carved so as to look like an upside-down, pointy pear. However, since nothing of the 2 figures appears to have been damaged as all detail seems to be intact, there might be nothing there to look for; compare the rather large tickness of the bas-relief of the bird (on the extreme left) still accentuated by the strongly aslant edge of the animal's head, back and tail. Finally, hanging down from the servant's right wrist by 2 straps (as also in CIH 419 and 445) is the master's handbag of which the round bottom recalls the duffel bag of the youth of CIH 445. Pirenne suggests the object to be "une sorte de bourse" (p. 119), an expression already used by C. Clermont-Ganneau for the object of CIH 445 (cf. l.c., 1870, p. 306; cf. above, p. 10). If lady Ḫulgat of CIH 419 prefers to keep herself her handbag, the large duffel bag of ʿAglum is given to the care of a youth as ʾAzʾad's small handbag of CIH 723 is cared for by an accompanying servant.

CIH 723 has in common with CIH 419 and 445 the 6 following features which justify my assumption that those 3 steles were made by the same carver:

1 - the palaeography of the 3 texts is the same although the letter form of CIH 703 is slightly thicker;
2 - the hairstyle of the 2 men is the same as that of the 2 boys of CIH 445;
3 - the lower half of the servant's garment has the same checkered design as the small boy of CIH 445;
4 - the very large ellipsis representing the left eye of the servant is the same as no. 10 of above, p. 33;
5 - the bird's body is studded with 6 small cunei, as the 5 in the lambkin of CIH 419 (cf. above, p. 12); and

6 - the left hand of the 2 men is in the same position as in no. 13 of above, p. 33.

From the viewpoint of the activities centered on animals, the material used in the previous pages may be presented as follows:

1 - camel driving: CIH 445 and LP 325;

2 - camel raiding: none;

3 - camel hunting: CIAS 972 and 1190; and

4 - irrealistic hunting drawings:

a - 5 javelins from below and 1 through the breast hit a standing mule in Anati, l. c., fig. 13 (p. 27) and pl. 10 (p. 29). The editor (p. 28) writes that "Five spears cross the body of an ass;" the verb hardly means anything; and he fails to point out the following amazing details: the 6 weapons are fully delineated; 5 javelins hit the beast from below and 3 of them are totally delineated inside the body, the 6th javelin hits the mule's breast;

b - one javelin hits the animal from above and is completely delineated in fig. 4 (p. 14) = pl. 4A (p. 13). Anati still uses the verb "to cross" without specifying the position of the 2 javelins seen on fig. 4 (p.12);

c - similarly, 4 javelins have penetrated into a camel's body from above and the right, and they are fully delineated in fig. 15 (p. 64) = pl. 8 (p. 69); the editor does not give any of the preceding details (p. 62);

d - same pattern, but a vertical javelin transfixes the sheep completely from above and is completely delineated in fig 2 (p. 19) = pl. 9B (p. 24); the editor (p. 9) still uses the same all-purpose verb "to cross;"

e - 2 even more irrealistic details are given in Atlal, 5 (1401/1981), pl. 34, viz

1) the shoe of the weapon is at a level lower than the hoofs of the animal; cf. C (right weapon), D (left, 2 weapons) and E (left, and right, central weapon);

2) - even in D (lower right), a legless cripple stands below a bovine he is about to hit with his javelin.

November 28, 1993.

Note to be added to p. 26, ✱ . - Drawings of abnormally bulgy camel humps are not rare in Saf; cf. with squarish angles, e.g., in WH 266, 424, 455, 1705, etc; with rounded edges, e.g., as in WH 362, 456, 1277, 1480, etc; with a trapezoid form, as in WH 2009.

II - Compte rendu de

C. Robin, Inventaire des inscriptions sudarabiques.

Tome I: Inabba', Haram, al-Kāfir, Kamna et al-Harāshif;
fascicules A: Les documents, et
B: Les planches.

Académie des Inscriptions et Belles-Lettres et Istituto Italiano per il Medio ed Estremo Oriente, Paris et Rome, juillet 1992, 22,9 x 17,5 cm, 223 pages et 60 planches, dont un dépliant.

L'examen critique suivant est le résultat, non d'une lecture systématique, mais bien de sondages effectués de-ci de-là, d'où furent écartées toutes les argumentations exigeant une longue recherche, ce qui a permis de passer en revue un plus grand nombre de questions sans imposer pour autant les proportions d'un livre.

1 - Préambule.

A - Ce qui frappe en tout premier lieu le lecteur, c'est l'adjectif "sudarabique" du titre, car il ne peut manquer de se remémorer la diatribe de M. Rodinson contre inconnu en faveur de cet adjectif où il publie sa profession de foi en ces termes: "je reste fidèle à l'école qui écrit systématiquement 'sudarabique'" (cf. J. Chelhod et al., Le peuple yéménite et ses racines, Paris, 1984, p. 60, note 17). Il est particulièrement caustique de voir le pourfendeur d'obscurantistes, M. Rodinson, - n'a-t-il pas écrit dans son Marxisme et monde musulman, Paris, 1972, p. 7, qu'"il y a des obscurantistes partout"? - plier le genou, faire la courbette et se cramponner mordicus à une appellation vieillotte sous prétexte - est-ce possible? - d'"ipse-dixitisme". Certes, une flatterie publique ne manque jamais d'être la meilleure flèche dans le carquois de celui qui désire une faveur or espère un oubli (cf. déjà plus haut, pp. 29-30). Malheureusement, ses arguments ne portent pas (cf. mes Miscellanées d'ancient arabe, Washington, XIV [1985], pp. 63-64). D'ailleurs, l'Académie des Inscriptions et Belles-Lettres elle-même ne pouvait en être bien convaincue puisqu'elle avait accepté le titre de "Corpus des inscriptions et antiquités sud-arabes" (= CIASA) de J. Pirenne en 1977 et avait renouvelé son approbation pour le volume II en 1986.

B - Le pamphlet lui-même ne justifie pas davantage la mention de l'Istituto Italiano ... aux pp. I (la couverture) et V du fasc. A = pp. I (la couverture) et V du fasc. B. En effet, l'introduction des 5 sites (pp. 3, 12, 129, 150-151 et 197) ne mentionne aucune présence italienne, même pas comme badaud ou curieux, c.-à-d. ceux qui se contentent de regarder. De plus, aucun nom italien n'est joint aux 2 organismes français "avec le concours de" qui "ce volume a été préparé" (cf. fasc. A, p. III = fasc. B, p. II). Enfin, le tome I fut "publié par les soins de Christian ROBIN" (cf. fasc.A, p. III = fasc. B, p. II) sans l'aide d'une quelconque bougie italienne. Aucun Italien,

membre ou non de l'Istituto Italiano ..., n'est donc impliqué dans la recherche archéologique et épigraphique des 5 villes, pas plus que dans la préparation, la rédaction ou le financement du tome lui-même. Comme le signale la p.3 de mes Miscellanées ..., XVII (1989), l'adjectif "Italiano", pourtant bien essentiel dans le titre officiel (voir aussi "Italiano" avec une majuscule dans la "Préface", p.1) n'est pas représenté dans l'abréviation protocolaire "IsMEO " (cf. aussi p. 120) = "ISMEO" (?!) in PSAS, 25 (1995), p. 182, qui reste toujours sans justification... et le sigle "IsMEO" continue son petit train-train journalier tout en se pavanant dans les publications italiennes et autres, apparemment inconscient ou peut-être même fier dans son origine bâtarde, une "combinazione" à l'encontre du bon sens, au lieu d'"IsIMEO." Mais, ici déjà, Robin fait barde à part et sans le faire remarquer, - ce que d'ailleurs les bonnes manières et la plus élémentaire gratitude lui interduisaient formellement, - il contredit A. Caquot, le bénévole auteur de la "Préface" de son volume. En effet, et peut-être en guise de réponse indirecte à ma critique - il a très bien pu vouloir remédier à la réponse inefficace de Rodinson (cf. la page précédente) -, Robin transcrit "Istituto italiano ..." (p.120) avec un "i" minuscule et, pour mieux enfoncer le clou de son obstination sottement aveugle, il récidive en donnant le titre du "Museo nazionale romano" (cf. l.c.) = MNR. Et ce n'est qu'un début: Robin donne ici un exemple des innombrables illogismes et contradictions qui saturent et, pour ainsi dire, étouffent les pages de son volume. En effet, le pompeux "MAFRAY: Mission archéologique française en République arabe du Yémen" (p. 206) prouve bien que chaque mot principal du titre est représenté dans le sigle par son initiale en majuscule, quelle que soit sa forme, minuscule ou majuscule, dans le titre; donc, la majuscule "F" du sigle correspond à la minuscule "f" de "française" du titre. Pourquoi en serait-il autrement pour le "I" d'"Italiano"? La réponse est simple: la folle du logis de Robin ne connaît jamais de repos ou de limite; elle est toujours en perpétuel mouvement dans un sens ou dans l'autre. Il serait impensable - c'est plus que manifeste et insultant même d'y penser - pour Robin ou un quelque autre membre de sa caste de Paris, Lyon ou ailleurs, de songer au sigle "MARAY" et plus encore de le préférer à "MAFRAY" bien qu'il soit même plus facile à prononcer.

C - La p. II, le revers de la couverture des 2 fascicules A et B, ne contient qu'une seule information, la légende de la photo imprimée à la p. I, avec une erreur orthographique (cf.p.114): "brûle-parfums conservé au British Museum (Londres)", un pluriel se référant à une seule unité!?!? Et la p. V du fasc. A = la p. IV du fasc. B proclame à la face du monde que le pamphlet de Robin, publié sous les auspices et le "patronage" (p. 2) de l'"Académie des Inscriptions et Belles-Lettres", et présenté au grand public par une "Préface" (cf. la page suivante) d'

" André Caquot
professeur au Collège de France, membre de l'Institut " (p.2)

" a été préparé avec le concours de l'Institut de Recherches et d'Études sur le Monde arabe et musulman (C.N.R.S), Aix en Provence, et celui de la Sous-Direction des Sciences sociales et humaines, Direction générale des Relations culturelles, scientifiques et techniques, Ministère des Affaires étrangères, Paris " (p. V).

Beaucoup d'encens est nauséeux, et le brillant de l'or et de l'argent des blasons des sponsors ne déteignent certes pas sur le contenu des pages pour lesquelles ces mécènes ont déboursé largement. Surtout aucune mention de l'"IsMEO" (cf. ici, p. 38).

D - Le lecteur peut lire dans la "Préface" d'A. Caquot (cf. plus haut) un témoignage définitif, aussi clair que succinct, en guise de conclusion, savoir

> " En donnant son patronage à la publication que signe M. Christian Robin, l'Académie des Inscriptions et Belles-Lettres entend renouer avec une belle tradition, qui est de mieux faire connaître le passé du Yémen, et témoigner de sa fidélité à un projet conçu en 1869 avec un enthousiasme non exempt d'illusions, celui de constituer un *Corpus Inscriptionum Semiticarum*, en le reprenant sur des bases plus modestes et plus sûres " (p. 2).

Voilà bien une proclamation d'une netteté aveuglante qui sonne le glas de l'entreprise de J. Pirenne commencée en 1977, savoir Corpus des inscriptions... dont le 2e tome est paru en 1986. Mes comptes rendus de ces 2 volumes (cf. Orientalia, 48 [1979], pp. 260-284, et mes Miscellanées ..., XVI [1988], pp. 11-43) indiquent que la relégation de ce CIASA dans la genizah de l'Académie, qui doit bien avoir son propre dépotoir pour les indésirables, les rejetés et les inutilisables, ne provoquera aucune jérémiade qui ne serait d'ailleurs que platonique.

Faisant allusion au premier Corpus de 1889, etc., la "Préface" semble prendre un malin plaisir à parler anachroniquement en 1992 d'un "projet conçu en 1869 avec un enthousiasme non exempt d'illusions". Cette remarque d'une naïveté mal placée, on va le voir, reste de mise pour la nouvelle et 3e fournée.

On était bien habitué au ronron des fantaisies de la 2e idole de la cella, et on savait à quoi s'en tenir et à quoi s'attendre. Le 2e changement d'aiguillage en moins de 25 ans est plus apparent que réel tout en étant nettement péjoratif. Avec le CIASA, le lecteur pouvait de temps à autre et à sa discrétion reprendre haleine et se reposer des développements toujours fastidieux et agaçants de Pirenne en lisant des sections rédigées par d'autres. Avec la nouvelle fournée, c'est un bourrage de crâne soutenu et impitoyable qui, bien que d'un genre tout différent de celui de Pirenne, est tout aussi brutal et écoeurant par sa mentalité partisane, son mépris de tout ce qui n'est pas sien, son imagination effrénée et le fanatisme de son pan-arabisme classique.

De plus, il ne sied guère de parler de "bases plus modestes" quand on compare la simplicité objective des translittérations du Corpus d'antan à celles trompeuses et compliquées au point d'être inintelligibles (cf. plus loin) dont Robin infecte impitoyablement toutes ses pages les unes après les autres. Et si les "bases plus modestes" se réfèrent surtout, voire même uniquement, au format des volumes, c'est bien là donner une importance capitale à des détails nettement secondaires n'ayant rien à voir avec l'essentiel, la seule chose importante, le contenu, sans lequel la couverture surchargée n'est qu'une coquille recelant un magma inépuisable d'erreurs et d'à priori de tout genre.

En outre, les notes suivantes montrent que "les bases ... plus sûres" qui sont épinglées comme la preuve irréfutable d'un progrès aussi apparent que certain et qui eussent été les bienvenues, ne s'avèrent guère être qu'un mirage éphémère, le fruit d'un rêve extravagant sans contact réel avec les publications antérieures et qui, comme il fallait s'y attendre, ne réflètent nullement le pamphlet annoncé. Qu'il me soit permis de renvoyer le lecteur aux remarques de mes Miscellanées ..., XVII (1989), pp. 93-94, sur un cas identique, l'attitude très ampoulée de G. Gnoli relative au volume édité par A. de Maigret, The Sabaean Archaeological Complex in Wādī Yalā, Rome, 1988. Cette même expression ne peut davantage viser la section imposante des illustrations dont une bonne majorité sont des photos. On se doit une nouvelle fois de reconnaître que, comme dans le passé (p.ex., sa thèse Les hautes-terres ... Istanbul, 1982), les photos de Robin normalement surexposées (p.ex., pl. 24 et suiv.) ne servent qu'à entraver le travail épigraphique et souvent même à le rendre impossible. QUEL CONTRASTE avec les splendides illustrations du premier et vrai Corpus sur les bases sûres duquel on pouvait toujours compter et qui, à ce point de vue du moins, avait trouvé un digne successeur dans CIASA de Pirenne! On commence à comprendre la tocade de Robin pour de Maigret, en qui il a trouvé enfin son alter ego dans l'art du fiasco photographique, de l'absence sur chaque photo, d'une échelle combien précieuse au lecteur, le renseignant sur les dimensions exactes de l'objet, et aussi d'une publication désinvolte des textes.

La question des mauvaises photos de Robin est un imbroglio que le lecteur ne s'explique pas. L'Académie a reçu - on n'en peut douter - une copie de chacune des publications de Robin; elle a donc eu tout le temps de se rendre compte de la mauvaise qualité de ses photos. Et qui oserait se permettre de faire à l'Académie l'affront ne serait-ce que de penser qu'elle ne s'en soit jamais rendu compte à moins, évidemment, que ces copies aient été fichées, classées et rangées définitivement dans une section spéciale du débarras de l'Académie sans que personne n'y jette un regard scrutateur et ne dresse un rapport sur la présentation, l'impression et la qualité des illustrations, ce qui serait à peine croyable, mais reste ... possible. De plus, on ne peut davantage douter que Robin ait soumis son manuscrit, les nombreux emprunts à J. Halévy et A. Fakhry et les photos

d'accompagnement à l'approbation du comité d'édition qui n'est tout de même pas un office dispensant les autorisations comme un distributeur automatique de timbres et de rafraîchissements. Il semble particulièrement inquiétant que même la "Préface" qui brosse un tableau idyllique d'une oeuvre qui ne peut être que parfaite, ne fasse même pas allusion à cette lamentable documentation photographique, si l'on exempte les photos de musée encore que, là aussi, tout ne soit pas normal, p.ex., pourquoi la photo de RÉS 4674 de pl. 10b est-elle aussi noirâtre alors que la photo du musée à ma disposition est bien claire? Dès lors, comment comprendre que toutes ces photos inutiles parce qu'inutilisables (p.ex., pl. 4a, 19b, 24a, 26a, 28b, 29a, b, 31a, b, etc) ou mal prises et inutilisables du point de vue épigraphique (p.ex., pl. 7a, b, 26b, 29a, b, etc) aient pu recevoir l'approbation de cette Académie? Dès lors, comment pouvoir espérer - mille fois hélas! - que cette même Académie soit disposée à envoyer sur le champ yéménite un photographe capable pour reprendre toutes ces photos publiées par Robin et fournir ainsi au monde scientifique ce qu'il était en droit d'espérer et qu'il était du devoir de Robin de lui donner? Le lecteur doit une fois de plus s'en remettre aux calendes grecques comme il a déjà dû le faire pour les photos publiées par de Maigret (cf. mes Miscellanées ..., XVII [1989], pp. 7-14). Pourquoi se leurrer d'illusions fantasmagoriques, quelque fascinatrices qu'elles soient ou puissent être?

E - Avant d'entrer de plein pied dans le pamphlet, il convient de signaler un fait bien curieux qui ne s'explique pas et n'est même pas signalé. Faisant immédiatement suite à la p. 2 qui contient la 2e moitié de la "Préface" d'A. Caquot, il y a 2 pages qui ne sont pas reprises dans la numérotation suivie puisqu'elles précèdent la 1ere page de texte, non numérotée à cause du titre et qui est la p.3, la suivante portant le n° 4. La 1ere de ces 2 pages ignorées porte le titre "Chapitre1 I N A B B A ʾ" au milieu de la page et la suivante est en blanc. Ces 2 pages ont été méconnues puisque leurs équivalentes des 4 autres sites sont bien les pp. 9-10, 127-128, 147-148 et 195-196. Qui était chargé de corriger les épreuves et de donner le bon à tirer: l endormi ou l ivrogne?

2 - Introduction.

Cette section propose 4 cas qui se veulent de préparer le lecteur à s'attendre à tout, y compris des argumentations caduques et des trompe-l'oeuil.

A - Une conséquence indéniable du fameux adage de justice distributive "Cuique suum" est que personne ne peut incriminer un auteur d'une erreur de lecture commise par un tiers. Cela paraît bien simple, mais pas pour tous. Pour une illustration toute récente, cf. mes Miscellanées ..., XVIII (1995), pp. 81-89A. Voici un exemple de pareille injustice. Robin mentionne le "Mtbmdgb de Höfner 1965, p. 517 (CIH 603 b/2)" (p. 180). Voilà donc Robin reprochant à M. Höfner, qui ne peut plus se défendre (ce qui manifeste également la bravoure impavide de l'accusateur), d'une copie erronée ... qui vient de J. Halévy (qui n'est pas mentionné, bien entendu!) dans JA, 19 (1872), p. 197,

no. 344/2, et fut reprise souvent depuis lors, p.ex., par H. Grimme dans OLZ, 9 (1906), col. 326.

B - La saga de mtbmdgb continue. Robin écrit: "Si mtb était une graphie sudarabique de l'araméen mytb ... siège" (p. 181). Toute la recherche étymologique relative à mtb est à ce point capitale aux yeux de Robin (p. 43) qu'il a tenu à en répéter les détails ... 2 fois, aux pp. 63-64 et 181. De plus, désireux sans doute de renforcer encore son étymologie grossièrement erronée , Robin s'en réfère (p. 64) à F. Hommel dans Florilegium ... Melchior Vogüé ,Paris, 1909 , pp. 299-300, une opinion que l'auteur avait déjà proposée en 1904 (cf. CIH, II, p. 213). À noter incidemment que le titre du volume dans lequel F. Hommel publia son article en 1909 est atrocement télescopé dans la bibliographie de Robin (p. 205) et plus encore (avec "Vogüe"!) sous la date erronée "1910" dans sa Bibliographie ... du CIASA, Louvain, 1977, p. 64, no. "14.1910." La suggestion de Hommel de 1904 était déjà erronée à cette date puisqu'E. Glaser avait publié le mot mwtb de Gl 1500/3 dans Die Abessinier in Arabien und Afrika, Munich, 1895, p. 48. Et J. Halévy avait lui aussi publié dans RS, 14 (1906), p. 367, l'expression mwtbhmw de BM 102458 - Robin ne mentionne ni Glaser ni Halévy, bien sûr -, qui est devenu CIH 396 en 1911; cf. aussi le pluriel mwtbt de Ja 575/5 (1962). Robin ajoute enfin que "l'élément nty^n n'est pas attesté par ailleurs" (p. 64). Cette lapalissade ne le dispense pas d'en rechercher une étymologie (cf. CIH, l.c.); mais il se garde bien de le faire. La raison de ce silence obstiné ne peut être que le fait de ne trouver aucune étymologie susceptible de confirmer le cadre actuel de ses idées préconçues ou de l'y insérer harmonieusement. Et cependant, Robin est à ce point hypnotisé par son propre prurit de transcription qu'il ne se rend même pas compte qu'en reproduisant son "nty^n", il a déjà choisi son étymologie puisque le n final est exclu de la racine, et la racine qu'il s'impose à lui-même est bien nty parce qu'il n'a aucune raison, sans chercher midi à quatorze heures, de retenir l'une de ces 3 racines, nwṯ, nyṯ ou wnṯ. Rien ne s'oppose à faire appel à l'arabe naṯiy "éloigné."

C - Le cas suivant illustre une fois de plus la superficialité de la recherche de Robin. Dans son introduction à CIH 523 (p. 109), Robin affirme que "Höfner 1944 ne permet pas de résoudre la contradiction" de l'équivalence de ce texte avec Gl 1051, 1052, 1652 ou 1789. Mais les sigles Gl 1051 et 1052 (de G. Ryckmans, dans Le Muséon, 58 [1945], p. 2) n'existent pas séparément; ce qui existe, c'est "1051/2" (cf. F. Hommel, dans E. Glaser, Altjemenische Nachrichten, Munich, 1908, p. 8A, gauche), qui est "un plat avec des cercles caractéristiques". Selon F. Hommel (cf. l.c., p. 13A, droite), A. Grohmann (cf. Göttersymbole ..., Vienne, 1914, pp. 6B, 39A-40A et fig. 90), M. Höfner (cf. Die Sammlung ..., Vienne, 1944, p. 57) et l'estampage de Glaser A 528, Gl 1652 est une ornementation anépigraphique composée du croissant lunaire et du disque solaire. Il ne reste donc que Gl 1789 que F. Hommel (cf. l.c., p. 15A, droite) décrit comme étant une des "Wiener Steine;" donc, Sem 6 = CIH 523, exactement comme A. Grohmann avait écrit à G. Ryckmans, une information que Robin a traitée avec insolence, mépris et dédain, c.-à-d. "à la Robin".

D - Alors que, dès le début de son pamphlet, Robin s'évertue avec acharnement à créer l'impression d'exprimer chaque parcelle de texte pour en extraire toute l'information possible, il est plus qu'étonnant de ne trouver dans toute sa prose absolument rien des populations minéenne et sabéenne et les influences qu'elles ont pu subir à la lumière de l'onomastique qui n'est pratiquement pas étudiée dans le pamphlet et tout particulièrement des noms théophores au dieu lunaire qatabanite ʿm ʿAmm, parmi lesquels on trouve ʿmʾmr, ʿmʾns (3 x), ʿmdrʾ (3 x), ʿmytʿ (2 x), ʿmkrb, ʿmʿhr et ʿmšfq (2 x). Et les pages suivantes illustrent le fiasco complet de l'entreprise de C. Robin.

3 - Bibliographie.

Le but primordial de la bibliographie (pp. 203-209) est clairement d'encenser la meute des amis de l'auteur - il serait plus exact de parler de complices politiques - parce qu'eux, et eux seuls, reçoivent les coups d'encensoir et les cadeaux de copies; p.ex., la mention d'une plaquette "sans nom d'éditeur;" cf. p. 203: "Breton ... 1990", un écrit mentionné à la p. 154. Dans le cas présent, les 3 éditeurs sont bien connus puisqu'ils sont de toute nécessité les auteurs; seuls restent inconnus le nom et adresse de l'imprimeur. Voilà donc une "plaquette de 24 pages (sans pagination)" - est-ce possible d'être aussi insouciants et aussi primates! - que le lecteur ne peut en aucun cas se procurer sans passer sous les Fourches Caudines de "Son Excellence Monsieur Jean-François Breton"... pour "Lui" demander la faveur, le cadeau, l'aumône ... d'un exemplaire. C'est là une courbette, une imposition de sujétion qui n'est qu'une insulte personnelle. Aucune mention de l'article de G. Gnoli signalé à la p. 47; même le nom de Gnoli est absent de la bibliographie (p. 204), de même que le sigle du texte qu'il a publié (p. 216 , où il n'y a aucun sigle commençant pas "Y"). Inutile de s'attarder pour dénoncer au monde scientifique digne de ce nom, qui reste encore libre de toute servitude politique, la manie du groupe méditerranéen du CNRS d'imprimer des plaquettes et pamphlets impossibles à se procurer faute d'adresse de libraire et qui ne sont distribués qu'à la clique de leurs potes;ces opuscules NE sont PAS publiés (cf. déjà mes Miscellanées ..., XVIII [1995], pp. 173-174) puisque, par définition, "publier" signifie "faire paraître", et "paraître" signifie "en parlant d'une publication, être mise en vente dans les librairies".

On remarque, en outre, que la collection de mes 17 volumes, totalisant

2632 pages et 164 planches et intitulée Miscellanées d'ancient arabe, Washington, 1971-1989, n'est pas renseignée pour la simple et obvie raison, cela va de soi, que c'est là où, depuis le début, quelques élucubrations imaginaires et quelques suggestions saugrenues de Robin, qu'il est plus aisé d'appeler "Robineries," ont été mises en évidence, car il est pratiquement impossible de les signaler toutes, voire même beaucoup. Et si Robin ne renseigne pas que l'exemplaire des Miscellanées ... cité à la p. 205 est le vol. II, c'est que, de toute évidence, il voulait vouer à l'oubli l'existence de cette série.

4 - Paléographie.

Le schéma paléographique de Kamna et d'al-Ḥarāsif (p. 153) s'inspire fortement et copie partiellement celui de la Paléographie ... de J. Pirenne, 1956, p. 100 (cf. plus haut, p. 14).

Les particularités des textes d'al-Kâfir sont laissées de côté par l'auteur qui aurait dû informer son lecteur - mais est-il capable de le remarquer? - du tracé liḥyanite des lettres l et m d'al-Kâfir 15 (p. 138 et pl. 30b); le tracé général du graffite de même que son contenu (cf. Ja 3327 de Qaryat al-Fâw) indiquent que son auteur minéen a séjourné longtemps parmi les Liḥyanites du nord-ouest de la péninsule.

Robin n'a pas davantage signalé la présence dans al-Kâfir 28/1, 32/1 et RÉS 2841 d'une ancienne forme de h aux traits latéraux légèrement concaves formant un V, qui se retrouve également dans h de Kamna 5/2, 3, 4 et 9/2 et 3 et dans le tiers inférieur de g de Kamna 5/2 et 7/2. Les traits latéraux rectilignes de h de RÉS 2751 et 2847/1 et de h d'al-Ḥarāsif 3/1 sont encore plus anciens.

Un autre aspect significatif du travail de Robin vaut son poids d'or. Il s'en réfère très souvent aux types paléographiques créés par J. Pirenne qui furent immédiatement rejetés catégoriquement dans mon compte rendu de sa thèse, intitulé La paléographie (1956), où il est démontré que ces types ne sont que des mirages imaginaires de l'esprit schématisant de Pirenne. Il faut, en outre, mettre en évidence que l'échafaudage historique de Robin se base toujours uniquement sur la typologie paléographique de Pirenne, alors que, au cours des 25 dernières années, de nombreux repères chronologiques obtenus par les analyses au C14 ont pulvérisé sa mini-chronologie, que j'ai été le seul à rejeter et à combattre dès le début. Le bluff et la fausseté de tout l'échafaudage de J. Pirenne étaient à ce point manifestes qu'une dénonciation totale s'imposait de toute nécessité. Toutefois, le nombre, la qualité et la puissance de ses supporters, tous amis de G. Ryckmans, tant à Louvain qu'à Bruxelles, Paris, et ailleurs, ne permettaient pas le moindre doute sur l'issue finale, savoir relever le gant équivaudrait à un suicide social. Je restais donc seul en lice, car personne d'autre ne ferait le travail, ni A.F.L. Beeston, ni personne d'autre ... Et tout seul, en pleine connaissance de cause, et conscient des conséquences bien pénibles qui en découleraient de toute nécessité, je me suis opposé à Gonzague Ryckmans, à son orchestre universitaire louvano-flamand (sous le bâton cinglant du "grandiloquens" Joseph Coppens) et aux 2 académies flamande et française pour stigmatiser l'imposture dans un compte rendu de presque 200 pages, paru le 5 octobre de l'année suivante, 1957. L'avant-dernier paragraphe de cet opuscule décrit très bien l'ambiance d'alors; le voici:

" Et que penser de l'eulogie dithyrambique de l'ouvrage, que G. Ryckmans a présentée à l'Académie Royale Flamande de Belgique, et sur le témoignage de laquelle cet organisme scientifique l'a accepté dans ses séries? C'est avec un profond et sincère regret que nous publions notre plaquette, mais la vérité a des droits imprescriptibles à la lumière, même et surtout lorsqu'un éteignoir convaincu de sa force soi-disant magique, essaie de la bâillonner au bénéfice de ce qui ne peut qu'être appelé contrefaction et erreur."(p. 176)

En outre, les types paléographiques de Pirenne sont à ce point liés à sa mini-chronologie qu'ils en sont inséparables. Quand, donc, Robin s'en réfère à un type paléographique de Pirenne, il ne peut pas ne pas souscrire à sa mini-chronologie. Et il est ahurissant et combien symptomatique que la 1ère référence de Robin à la typologie paléographique de Pirenne renferme une contradiction manifeste, à moins que, comme je l'ai déjà suggéré auparavant, les mots aient perdu leur signification propre pour Robin. C'est à la p. 5, dans ʾInabbaʾ 1, qu'il écrit que la graphie du texte

> "appartient à la phase B définie par Jacqueline Pirenne mais avec des caractères qui ne correspondent précisement à aucun des stades que ce savant a décrits".

Cette déclaration revient à ceci: la graphie du texte appartient bien au B de Pirenne, mais aucune des caractéristiques de style, telles que les a déterminées la géniale inventrice dans les sept groupes qu'elle prétend avoir été fort heureusement à même d'identifier, savoir, B1, B2, B2', B3, B3', B4 et B4' (cf. Paléographie ..., pp. 127-135), ne se retrouve dans le texte étudié. Pareil illogisme, digne de Pirenne et typique de la jonglerie acrobatique de Robin, rend possible n'importe quel parallèle, n'importe quelle comparaison.

Un bel exemple de l'asservissement de Robin aux créations épigraphiques de Pirenne est sa paléographie des inscriptions de Haram (pp. 23-24). Il s'agit ni plus ni moins d'une perte sèche de temps qui, sous le couvert d'un parasol pseudo-scientifique, met au rancard - ici aussi - les particularités intéressantes des textes. Il eût utilisé son temps d'une manière profitable en signalant, p.ex., le d à 2 barres horizontales centrales dans CIH 530 (p.99 et pl. 10a), les médianes concaves de b dans CIH715 (p. 93 et pl. 8b), 530 et RÉS 4674 (p. 105 et pl. 10b) et des s et ṣ dans CIH 530, et des médianes convexes des ḥ et h dans CIH 530. Les médianes concaves et convexes de ces textes, qui ne sont pas utilisées par J. Pirenne très certainement parce qu'elles ne ca-

drent pas avec ses inventions typologiques, méritaient d'autant plus une mention qu'el-
les sont devenues une caractéristique du style de Qaryat al-Fâw aussi bien dans les tex-
tes sabéens que Fâwite (cf. mes Miscellanées ..., XVIII [1995], pp. 128-129).

5 - Inscription et photo.

Le résultat méthodologique le plus précieux et le plus manifeste du pam-
phlet de Robin est que l'auteur lui-même apporte la preuve la plus cinglante - personne
n'eût jamais pu imaginer un démenti aussi cuisant, aussi humiliant et aussi complet, ce
qui prouve bien le dérapage intellectuelde Robin - et dont le lecteur n'avait en aucun
cas besoin, de l'absurdité d'un de ses dogmes épigraphiques ... tout en se gardant bien
d'en rappeler le souvenir. Certes, il est des stupidités qu'il est préférable, en en
feignant l'oubli, de taire au lecteur. Il convient donc de rafraîchir la mémoire de Ro-
bin tout en informant le lecteur puisqu'il n'a pas eu le courage de le faire lui-même.

Robin a défini une des bases de son travail épigraphique en ces termes:

"une publication d'inscription sans photographie est inutile"

dans sa thèse dactylographiée sur "LE PAYS DE HAMDĀN ET HAWLĀN QUDĀ'A (NORD-YEMEN) AVANT L'ISLAM"

Thèse préparée sous la direction de M. le Professeur Maxime RODINSON et presentee en vue de l'obtention du grade de Docteur de Troisième Cycle, COLLÈGE DE FRANCE, Institut d'Études Sémitiques Anciennes, n° d'inv. 4715," Paris, 1977, p. 390, ll. 7-8.

L'affirmation précédente est reprise mot à mot dans le texte définitif (revu, corrigé et édité) de la susdite thèse sous le titre nouveau de LES HAUTES-TERRES DU NORD-YEMEN AVANT L'ISLAM, Istanbul, 1982, II, p. 90, ll. 11-12, et dans I, p. IX, Robin donne à ses lecteurs quelques détails supplémentaires sur sa thèse qui fut

"soutenue le 27 mai 1977 à l'Université Panthéon-Sorbonne (Paris I). Le jury était compose de Paul Garelli, président, Maxime Rodinson, rapporteur, André Caquot et Jacques Ryckmans".

Mes Miscellanées ..., XIV (1985), p. 279, ont déjà remis les choses au point en déclarant que "Robin is manifestly dishonest because he does exactly what he condemns," ... et cette affirmation est suivie de la liste des textes publiés par Robin SANS PHOTO dans la même thèse et aussi dans Semitica et Raydān, II. La situation dans le récent pamphlet est combien plus scandaleuse et lance un défi de rébellion hargneuse à l'honnêteté la plus fondamentale. Voici le schéma de la situation telle que son pamphlet la présente (voir en haut de la page suivante). Robin traite de 117 textes, dont presque la moitié, soit 56, proviennent de Haram et sont les plus longs et les plus importants, et 28 de ces 56 textes n'ont jamais été photographiés, ... et les photos de 2 autres ne sont possibles que sur des estampages. À ces 28, il faut en ajouter 7 autres, ce qui porte à 35 le nombre des textes qui, sur les 117, ne sont pas illustrés par une photo et ne peuvent l'être; donc, la proportion est à peu près 1/3.

	Textes	Photos directes	Photos d'estampages	Photos de fac-similés	Aucune photo
Haram	56	27	2	2	28
ʾInabbaʾ	4	4			
al-Kâfir	32	32			
Kamna	22	15			7
al-Ḥarâsîf	3	3			
	117	81	2	2	35

La conclusion est manifeste et inéluctable:

si Robin avait eu un grain de logique, il lui eût été impossible de considérer même la possibilité de son projet; et

s'il avait eu un grain d'honnêteté, il n'eût certes pas manqué une occasion aussi astreignante de désavouer publiquement son aberration.

Sa double capitulation est malheureusement un fait accompli et inexorablement immuable, dont il ne pourra jamais ni effacer le souvenir ni récuser la paternité et qui le flétrit à tout jamais de mensonge et d'hypocrisie.

6 - Déchiffrement.

Le travail de déchiffrement de Robin dépend de son principe fondamental de suffisance personnelle: s'il se voit incapable de lire une lettre sur une photo, quelle qu'en soit la qualité, même si cette lettre a été lue avant lui, elle n'en sera pas moins considérée comme douteuse ou surmontée d'un petit cercle, si tel est son bon plaisir qu'il ne doit d'ailleurs - telle est sa conviction - ni expliquer ni justifier à personne; p.ex., les 10 lettres de CIH 523 (p.110) qu'il fait surmonter d'un petit cercle dans son texte sont toutes certaines dans CIH (II, p.237; les 32 séparateurs sont supprimés), comme il aurait pu aisément le constater sur la reproduction du texte sud-arabe du CIH, parue dans les Mahtârât min an-nuqûš al-yamanîyat al-qadîmat, Tunis, 1985, p. 149) où son nom figure comme 3e coéditeur; la lecture de ces lettres ne fait pas davantage de doute pour tout épigraphiste, qui a étudié l'original à Vienne, une recherche que Robin n'a pas eu le courage de faire. Cf. aussi ici, pp. 40 et 183.

Cette attitude de Robin est d'autant plus irrationnelle et égoïste que son travail de déchiffrement doit être contrôlé systématiquement, et tout spécialement les inscriptions d'al-Kâfir qui ne seront jamais éditées définitivement qu'après la publication de bonnes photos et du déchiffrement des originaux par un épigraphiste digne de ce nom. En un mot, Robin n'a pas fait son travail, n'a pas rempli sa mission. DEUX exemples montrent que, comme par le passé et en règle générale (cf. déjà mes Miscellanées ..., X [1979], pp. 54-55 au sujet des graffites d'al-Ḥazâʾin), Robin base ses lectures sur des photos, et non sur les originaux, comme l'a toujours fait J. Pirenne avant lui (cf. aussi ici, p. 233).

a - al-Kâfir 30 (p. 145): Robin ne lit que ce qui est visible sur la photo de la pl. 38a. Des 14 lettres que comporte son déchiffrement, un m et un trait de séparation des mots sont déclarés certains; toutes les autres lettres sont surmontées d'un petit cercle ou insérées entre 2 parenthèses, sans que le lecteur puisse connaître ou même deviner la différence entre les deux ... car, comme il est dit plus haut, Robin ne se doit ni d'expliquer ni de justifier ses caprices à personne (p. 47).

b - al-Kâfir 31 (pl. 38b): selon Robin (p. 145), il s'agit d'un "graffite dont il n'a pas été possible de faire une lecture complète en raison de l'ombre portée par un surplomb du rocher." Quel scandale! Une vulgaire lampe de poche, qu'on n'a aucune peine à se procurer à Ṣanʿâʾ et ailleurs et qui est une nécessité absolue où que l'on se trouve ou aille, eût suffi pour lire le texte dans l'ombre, comme c'est le cas du 2e panneau des listes onomastiques du J. Balaq Janûb, Ja 2849 c-f dans mes Miscellanées ..., X (1979), pp. 1-20 et pl. 1-4, que j'ai dû lire lettre par lettre grâce à une lampe électrique, un travail fatigant et fastidieux pour le chercheur accolé à une échelle qu'il doit déplacer à chaque lettre. Mais, la plus puissante des lampes n'est d'aucune utilité sur une photo qui ne montre qu'un magma confus d'aspérités et de cavités imprécises et grisâtres.

Allongeons de plusieurs numéros la liste des exemples de lecture de Robin.

c - Le nom initial d'al-Kâfir 6 (p. 134 et pl. 26b) est lu "(ḫ)s¹(k)-" avec le commentaire suivant: "(ḫ)s¹(k): le k est très incertain mais le ḫ a été lu sur la pierre." C'est la logique caractéristique de Robin de métamorphoser son "s°1" en "s¹" et enfin en "s" dans sa translittération, son commentaire et sa traduction; voir aussi la disparition du trait d'union après son "(k)". Toutefois, même si, exceptionnellement, la lettre ḫ a été lue sur la pierre (mais comment y croire?, et où en est la preuve?), Robin semble à ce point conscient de son impéritie que sa lettre est mise entre parenthèses exactement comme le "k... très incertain;" mais, par un illogisme qui lui est bien caractéristique, ces parenthèses disparaissent dans sa traduction! Et la valeur du trait d'union reste inexpliquée!

d - L'incompétence précédente est également visible dans al-Kâfir 2b (pp. 131-132 et pl. 21b) où le lecteur apprend que "le second texte (B), dans l'ombre, ne se voit pas sur la photographie" (p. 132). Robin en est même arrivé à se persuader qu'il a trouvé l'excuse irréfragable de n'avoir rien fait; et il offre au lecteur 3 lettres dont il ne sait que faire, dont il n'est même pas sûr puisque la 1ère lettre est mise entre parenthèses et les 2 autres sont surmontées d'un petit cercle et, une fois encore, quelle est la différence entre ces 2 gadgets, le lecteur l'ignore. Finalement, il se garde bien de tenter une quelconque analyse du produit de sa conjecture. Est-ce possible que le CNRS et l'Académie des Inscriptions et Belles-Lettres en soient arrivés à accepter,

promouvoir et garantir de leur approbation officielle un pamphlet dans lequel l'auteur avoue ouvertement - ce qui semble être normal pour lui - n'avoir même pas eu recours à une aide aussi banale qu'une simple lampe de poche pour assurer la lecture des lettres tracées sur une section ombragée de la roche? Comment y croire? Et pourtant, c'est vrai.

e - Le nom initial d'al-Kâfir 9 (p. 135 et pl. 28A) est wslm Waslum (p. ex., Ja 2716 a) et nullement "w(ʾ)l^{m}" de la translittération (ici aussi, les parenthèses disparaissent dans la traduction!), comme le suggère Robin qui affirme erronément que la 2e lettre "peut être un ʾ, un s¹ ou un k". Cette remarque prouve son incapacité de distinguer la moitié supérieure de la lettre.

f - Le début du nom initial d'al-Kâfir 27A (pl. 36b) est lu "d dhu-" (p. 143). Il s'agit en fait du symbole divin déjà attesté dans CIH 530/1 (p. 99) et 532/1 (p. 100). Pour le trait d'union, cf. plus haut, p. 48, c.

g - Le nom initial d'al-Kâfir 32 (pl. 39a) se lit whbʾl Wahabʾil, et nullement "whb(t)" de Robin (p. 146); ici aussi, les parenthèses disparaissent dans la traduction et le commentaire qui affirme pompeusement que "whbt" (sans parenthèses!) est ici "comme dans RES 3763". Cette lecture erronée a été corrigée en ḏhbt dans Ja 2300 g; cf. mes Miscellanées ..., VII (1974), p. 77 et tracé à la pl. 20. Il est déjà mentionné plus haut (pp. 43-44) que Robin ne signale qu'un seul tome de cette collection dans sa bibliographie, sans en donner la tomaison; il s'agit d'un tome différent. En outre, Robin ne se préoccupe pas de signaler que la lettre r est gravée plus haut que les 3 lettres suivantes de la l.2, son axe horizontal étant à leur extrémité supérieure. Cette lettre r est au même niveau que le d initial parce que la section inférieure de l'espace que ces 2 signes auraient dû occuper sur la pierre, était endommagée.

h - Le nom de Kamna 3 (p. 171), dont la photo (pl. 47b) est imprimée sens dessus dessous (!?), se lit ʾbṣrb ʾAbṣarab; cf. le nom propre qatabanite ṣrbt de Ja 2898/3. Robin lit la 3e lettre en ṣ; il n'a vu que la moitié gauche du tiers inférieur de la lettre.

i - Une autre preuve de la lubie épigraphique de Robin est son déchiffrement de Kamna 4 (p. 172 et pl. 48a): les 16 lignes inutiles consacrées à la discussion de sa lecture prouvent simplement son ignorance crasse de ce genre de graffite. Comme le dit l'adage: il se noie dans un verre d'eau! Le nom se lit de droite à gauche comme l'exige la seule lettre d qui en indique la direction, savoir /df^{f}ʾ^{1} Dafi(ʾ), qui est connu comme le nom d'une vallée dans Ja 658/15.

j - al-Kâfir 24 (p. 142 et pl. 35A). - Si la longueur du texte est de "10,5 cm" selon Robin, la hauteur des lettres, qu'il ne donne pas, est de ± 3.6 cm. Robin la lit la 3e lettre "f" sur référence à al-Kâfir 31C où le nom de ce graffite-ci est donné

comme "douteux ("peut-être" de p.145). Mais la photo de ce dernier ne montre aucune ligne horizontale médiane. - Le nom se lit (Ky)wš (Kay)waš; cf. le nom qatabanite kyšm de Ja 1208/1, et l'arabe kâša (o)"être saisi de frayeur et trembler"; (ky) reste douteux sur la photo. — Mention est faite plus haut (p. 47) de la situation malheureuse des textes très courts d'al-Kâfir, dont 8 viennent de faire l'objet de remarques. Il faut y ajouter une remarque qui confirme encore la certitude que Robin traite cette collection de graffites comme appartenant à une classe inférieure indigne de l'attention accordée aux autres, réaffirmant ainsi son adhésion à la position générale de J. Pirenne, A.F.L. Beeston, J. Ryckmans, W.W. Müller, etc., sur le monumental et, corrélativement, la réclusion dans la préhistoire ou le thamoudéen, de toute graphie qui n'est pas moulée ou calquée sur ce monumental, comme il l'a d'ailleurs déjà fait en parlant de "qaḥṭānite" à l'occasion des graffites de la région de al-Ḥazâ'in. Il s'agit de la transcription des textes en lettres sud-arabes. Les textes ainsi transcrits en lettres sud-arabes peuvent être très courts, se composant seulement de

5 lettres: RÉS 3331 (p. 116);
4 lettres: CIH 795 (p. 111), RÉS 3330 (p. 115) et 3471 (p. 117);
3 lettres: Haram 1 (p. 61), et même seulement de
2 lettres: 'Inabba' 2 (p. 6).

Que s'est-il passé au début de la rédaction de la section consacrée aux textes d'al-Kâfir? Le pamphlet ne le dit pas et n'y fait même pas allusion. Robin a dû bien vite se dégoûter de composer en caractères sud-arabes des petits noms qui, au fond pour lui, ne valent pas grand'chose, si tant est qu'ils vaillent quelque chose. Quelle que soit l'explication qui ne sera évidemment jamais divulguée, voici les faits.

Dans al-Kâfir 1-7 (8 textes), les numéros 2B et 6 ne sont pas transcrits en lettres sud-arabes; mais
dans al-Kâfir 8-32 (39 textes), plus aucun texte ne l'est.

Donc, sur un total de 47 textes, 6 seulement des 8 premiers sont transcrits en lettres sud-arabes!

7 - Système de translittération.

Le système de translittération inventé par Robin fut toujours depuis le début rejeté dans mes Miscellanées ..., p.ex., VIII (1979), p. 25: "a novel system of transliteration which is as inaccurate as it is illogical and which ... paraît illustrer l'adage 'compliquer pour le plaisir de compliquer'"; cf. aussi l.c., XI (1979), pp. 44-47, XIV (1985), p. 269, et XVI (1988), p.47. Pourquoi Robin abandonnerait-il son idiotie puisque l'Académie l'a intronisé dans la cella académique et paie tout (cf. aussi ici, p.90)?

Ce système trahit le texte original parce qu'il le représente en fonction de son interprétation par l'auteur et nullement de l'original; et la transcription qui en résulte est rébarbative et erronée parce que tous les trucs ou gadgets utilisés, quels

qu'ils aient été, soient ou puissent être dans le futur, n'existent pas dans l'original qui doit être retranscrit tel quel; et certaines parties de l'original sont même éliminées, et ' est à ce point dénaturé qu'il devient une tache d'encre (cf. ici, p.181).

a - Parmi les trucs utilisés, il faut noter l'isolement des pronoms, des conjonctions et des formes verbales, 2 flanquant 1 ou 2 lettres 5 fois aux pp. 75-85, le # 3 fois aux pp. 140-141, la transcription en apostrophe des lettres -n et -m finaux, mais non de -t, comme si les dérivations nominales qtln, qtlm et qtlt n'étaient pas toutes issues du même et unique principe de qtl + une autre lettre en affixe. Comment ne pas y voir des manifestations ininterrompues d'un prurit de dissection à ce point tyrannique qu'il ne peut être que congénital? De plus, Robin introduit une distinction supplémentaire (comme s'il n'y en avait pas encore assez!) dans sa vocalisation des 2 dérivations nominales qtln et qtlm qui se retrouvent pratiquement à chaque page de son pamphlet. Voici les exemples du 1er texte qui présente les 2 cas, CIH 512 (pp. 69-70):

"Rym^{n} Mtbnṭy^{n}" et "Raymān Matabanaṭyān" et
"Wd^{m} Hrm^{m}" et "Wadd^{um} Haram^{um}".

Les affixes -n et -m paraissent bien être des appliques de plafond dans la transcription, mais par un ridicule "Robinien" caractéristique, le -n retombe sur le plancher des vaches dans la traduction tandis que le -m reste un soffite permanent ancré au plafond dans la translittération ... et jamais Robin n'a expliqué son illogisme qui ne peut venir que d'un pan-arabisme classique aussi obtus qu'écoeurant: la forme nominale en -ân est bien connue en arabe (donc, Robin l'accepte à bras ouverts) tandis que celle de -m reste inconnue (donc, Robin la relègue dans le dépotoir du plafond).

Cet illogisme ne peut, hélas, que l'induire tôt ou tard en erreur. Un seul exemple suffit ici (cf. déjà plus haut, p.42, le cas de nṭyn): le nom de lieu nšn (p.ex., CIH 516/7 et 7-8) est transcrit par Robin "Ns^{2}n", qui devient "Nashan" (pp. 1, 4, 52 et 84). Qu'il est stupide ce snobisme d'utiliser 2 transcriptions différentes du même signe!; cf. plus bas, c. En outre, la 2e est erronée étant donné l'absence de la racine nšn; c'est pourquoi CIH (p.ex., II, p. 138) transcrit le nom en "Našân." Cf. la note additonnelle de p. 98. - Deux remarques s'imposent au sujet du -m.

(1) Le -m ne file au plafond que s'il est la dernière lettre du nom; p.ex., les 2 exemples cités plus haut.

(2) Mais il reste sur le plancher des vaches s'il se trouve à l'intérieur du nom lui-même; p.ex. "'bm'ttr Abum'athtar" de RÉS 2740/4 (pp. 73-74) auraient dû logiquement être transcrits 'b^{m}'ttr 'Ab^{um}'attar," et
"Dhzm^{m} Ḍakhzam^{um}" de RÉS 2743/2 (pp. 77-78) auraient dû logiquement être transcrits dhz^{mm} Ḍahaz^{mum}" puisqu'il s'agit de la dérivation qtlmm; cf. p.ex.

les noms suivants: hlkmm de CIH 710/2-3,
ᶜsrmm de RÉS 4974/1, et
dgᶜmm de RÉS 3566/27. Ce nom retenu dans RÉS 4924/2-3 par RÉS, VIII (1968), p. 232A, doit être corrigé en dgwlm selon la photo à ma disposition, qui est une dérivation gtwlm; G. Garbini ne parle pas de ce nom dans AION, 33 (1973), p. 436 et photo à la pl. 4a.

b - Les éliminations. - Le système de translittération de Robin élimine les séparateurs des mots. Toutefois, ici encore et pour des raisons qui n'ont jamais été données - car, il faut le répéter (cf. plus haut, p. 47), Robin ne doit d'explication à personne! - son illogisme réapparaît vraisemblablement parce qu'un autre prurit, celui de l'inconstance, ne peut rester longtemps inactif ... et le séparateur des mots, remontant de l'abysse des soutes de Robin, refait surface dans la translittération; p. ex. Haram 2/2 (p. 62), CIH 512/4, 7, 9 (p. 69), 546/6 (p. 72), 530/7 (p. 99), 547/1, 5 (p. 75), 548/2, 7 (p. 79), etc.

c - Les lettres spécialisées. - L'instabilité de Robin, spécimen idéal de l'anthropoïde caméléon (cf. ici, pp. 80, 83 et 89), se manifeste en outre à propos des lettres spéciales sud-arabes utilisées en translittération: $\underline{s^1}$ $\underline{s^2}$ $\underline{\underline{t}}$ $\underline{\bar{g}}$ $\underline{\underline{d}}$ $\underline{h,}$ et
en traduction: s sh th gh dh kh.
Ici encore, qu'il est ridicule et stupide ce prurit de l'imbroglio ... qui complique pour le plaisir de compliquer et est responsable du "$\underline{\ }^1$" de "s^1" puisqu'il disparaît dans la traduction, tout comme les parenthèses (cf. plus haut, p. 48)! De plus, dans les 5 derniers cas, aucune indication n'est donnée de l'existence d'une seule lettre et non de 2, car Robin se garde bien d'emprunter le système de souscrire les 2 lettres d'un tiret horizontal, comme l'a toujours fait The Encyclopaedia of Islam. Mais, de grâce, ne commettons pas la faute de confiance béate. En effet, les choses changent dans la carte de la pl. 1. La date de ces changements reste inconnue. Il est, cependant, bien certain que le prurit de l'instabilité s'est une fois de plus évadé des profondeurs miasmiques du cerveau de Robin, et le voilà renversant la vapeur dans la carte jointe à son pamphlet (cf. la page suivante) pour les 4 questions suivantes.

1 - Les 2 signes accouplés pour former une des 4 dernières lettres (voir plus haut) sont maintenant souscrits d'un petit tiret horizontal, comme dans The Encyclopaedia of Islam, pour indiquer qu'ils ne représentent qu'une seule lettre;

2 - la lettre initiale de l'article arabe devient une capitale;

3 - une voyelle longue est indiquée par un accent circonflexe, et non plus par un tiret horizontal; p.ex. "Madhāb" (p.ex., p. 3) = "Madhâb" (pl. 1) et
"Nashan" (p.ex., p. 157) = "Nashan" (pl. 1).

4 - Le comble de l'ineptie est que cette carte créée tout spécialement pour illustrer le pamphlet dont le titre se compose uniquement de 5 noms propres omet de mentionner les noms des 3 derniers, c.-à-d. al-Ḥarâŝif,
al-Kâfir et
Kamna.
Cela n'empêche d'ailleurs nullement Robin, qui ne sait vraiment pas ce qu'il fait, de se référer erronément à sa pl.1 aux pp. 197, 129 et 149 pour al-Ḥarâŝif, al-Kâfir et Kamna.

Cette même carte a été reprise et considérablement farcie et bourrée de détails à en craquer dans la fig. 39 (p. 100) de J.-F. Breton, Les fortifications d'Arabie méridionale du 7e au 1er siècle avant notre ère (Deutsches archäologisches Institut Ṣanᶜā', Bd. VIII), Mainz, 1994; le titre de cette carte est répété aux pp. 3, 11, 129, 149 et 197 du présent tome et son titre complet figure à la p. 204. La lecture de cette carte exige de toute nécessité l'utilisation d'une loupe en raison non seulement de l'exiguité de certains noms mais aussi du malencontreux usage de traits horizontaux continus ou discontinus pour marquer des régions et qui, trop souvent, rendent très pénible le déchiffrement des noms. Il eût suffi à "Madeleine SINTES" de la pl. 1, qui devient "Madeleine Sintès-Aïoutz" de la p. 204 et est la responsable de la conception de cette fig. 39, d'utiliser les hachures obliques vers la gauche, continues ///, ou discontinues, ///, mais aussi de larges points au lieu des minuscules points souscrits qui n'apparaissent qu'à peine. De plus, les accents circonflexes sont ridiculement petits et se distinguent mal des points ordinaires (cf. l'accent au-dessus de la 2e voyelle de "Kuna"); certaines lettres sont trop rapprochées les unes des autres (cf. "Al-ᶜArîf" ou "Al-ᶜAnf"); certains noms ne se lisent pas sur la carte et l'"Index toponymique" des pp. 193-195 est par trop incomplet pour être d'une utilité quelconque; cf. le nom du wâdî au nord-ouest de "An-Nîr" (p. 100) = "an-Nīr" (pp. 5 et 16) = "(an)-Nīr" (p. 194B) est en fait Wâdî "as-Surayra" (p. 16) qui ne se lit pas sur la carte, pas plus que le nom du wâdî au nord-ouest de "Al-Bayḍâᵓ" (p. 100) = "al-Bayḍāᵓ" (p.ex., p. 3) = "(al)-Bayḍāᵓ" (p. 193A). -- Cette carte est mal conçue, mal réalisée et doit être refaite.

Somme toute, le cas de Robin est, pourrait-on dire, une réincarnation prématurée et combien amplifiée de la girouette déboussolée d'A.F.L. Beeston.

Il faut également souligner la stupidité de ce système qui, pour transcrire la lettre h, introduit une autre lettre k qui existe indépendamment et qui, pour être reconnue dans sa "valeur ajoutée" doit être accouplée à un h sans que rien, cependant, n'indique qu'elles ne forment qu'une seule lettre. Par contre, l'intervention spéciale d'un djinn bien attentif raviva dans le cerveau de Robin le souvenir des 5 phénomènes

vraiment spéciaux, savoir ḫ, ṯ, ṣ, ẓ et ḍ parce qu'il n'y a qu'un petit point souscrit; en conséquence de quoi, aucune différence n'intervient à leur sujet dans les translittérations et traductions de Robin. Le djinn a-t-il révélé à Robin la raison de cette exception? Celui-ci n'en dit rien, car, comme d'habitude, il ne se doit de fournir aucune explication à personne (cf. above, pp. 47 and 48).

Le lecteur n'a pas manqué de remarquer que Robin utilise toujours le mot "transcription" pour introduire sa vermine graphique des textes (p.ex., pp. 61, 62, 67, 69, 70, 71, 72, 73, 75, 76, 77, 79, etc) et jamais "translittération". Ce qui est malheureux, c'est qu'il a tort quelle que soit la définition qu'il préfère retenir. Car, "transcrire" est "reproduire exactement ... avec des caractères différents" ou "notation des mots d'une langue dans une autre", qui est le synonyme de translittération qui signifie"faire correspondre à (un signe d'une écriture) un signe d'une autre écriture". Il est bien certain que ni l'un ni l'autre de ces deux mots n'autorisent en aucune manière l'introduction et le pullulement à saturation des surcharges qui empêtrent ses translittérations au point de les rendre illisibles et répugnantes. Le mot que Robin eût dû utiliser au lieu de "transcription" est sans aucun doute possible "parodie",car chacune de ses translittérations en est réellement une.

d - Les chiffres.

Le lecteur ne trouve nulle part un exposé, même succinct, des signes numériques sud-arabes vraisemblablement parce que l'auteur en disserte dans C. Robin, L'Arabie antique de Karib'îl à Mahomet (cf. déjà,p.1),publié en juin 1992, donc un mois avant le présent volume. On trouve donc le subterfuge de translitterer 𐩱 = 100 et 𐩲 = 50 respectivement, comme "M" en majuscule italique (p.ex., p. 63, dans Haram 2/20) et "m" en minuscule italique (p.ex., p. 77 dans RÉS 2743/10) comme si une majuscule valait deux fois plus qu'une minuscule! La question des signes numériques est discutée dans mon compte rendu de ce dernier volume (à paraître) ; cf. Miscellanées...,XVII(1989),pp.81-83.

e - Évaluation finale.

Il faut à la vérité de dire que Robin a fourni un très gros effort et s'est surpassé lui-même dans son pamphlet. Jamais dans le passé, en effet, il n'a diversifié aussi abondamment son arsenal de surcharges qui fourmillent dans chaque ligne de ses translittérations et traductions et grouillent dans le reste de son pamphlet. Toutefois, je l'ai répété à satiété dans le passé et même dans les pages précédentes, c'est toujours avec le même résultat démoniaque de ne jamais expliquer la valeur exacte de chacun de ses gadgets. Quelle est la différence entre le point, le petit cercle surmontant une lettre et les parenthèses entourant la lettre, et aussi les crochets et les V couchés sur le flanc encadrant, eux aussi, une lettre (cf. plus loin)? Il est bien certain, toutefois, que ne rien préciser a, cela va de soi, le grand avantage de rester dans le

vague, de n'entraver en rien la liberté de changer ou de modifier à volonté et sans mot dire la valeur de chaque gadget, et surtout - autre gain très appréciable - de prévenir toute comparaison capable de dépister les illogismes, voire même les contradictions d'application.

8 - Traduction.

Les interprétations de Robin relèvent d'une imagination maladive de girouette en perpétuel mouvement; p.ex., mrtd qu'il traduisait par "confié" dans Les hautes-terres ..., 1982, I, p. 91, et "personne confiée à la divinité" à la p. 184, devient maintenant "dévot" (p. 144), qui est le sens de ʿbd. Il convient de noter que la phrase de Robin, "Il convient de corriger en ce sens Robin 1982-I,p.91", ne concerne nullement la traduction du mot, mais uniquement sa provenance. Pareil exemple montre que le mot d'ordre est bien de s'attendre à tout et de ne croire à rien.

En outre, la méthode de raisonnement et d'argumentation de Robin a déjà été dénoncée dans mes Miscellanées ..., X (1979), pp. 34-70 et surtout le résumé des pp. 65-66. Mes Miscellanées ..., XIV (1985), pp. 271-303, continuent à allonger la liste des exemples, qui n'en finit pas et s'allonge démesurément à chaque publication. Le cas de RÉS 2752 (p. 93) peut être présenté ici en guise d'introduction.

La section intitulée "Déchiffrement" (cf. plus haut, p.47) s'ouvre sur la constatation de la suffisance personnelle répugnante de Robin concernant la lecture des lettres. Cette même suffisance se retrouve dans ses traductions de texte, comme le prouve RÉS 2752 décrit par Robin comme "fragment inintelligible". Voilà comment il piétine l'éthique professionnelle,qui est régulièrement bafouée par les auteurs modernes, et qui lui imposait de signaler qu'A.F.L. Beeston avait proposé une interprétation de ce texte fragmentaire dans Le Muséon, 63 (1950), pp. 54-55. Robin ne peut même pas prétexter qu'il s'agit d'une traduction "vieillie" (cf. sa Bibliographie ... du CIASA, Louvain, 1977, p.6, note 1) puisque son auteur, de concert avec ses 3 suppôts, n.a. al-Ghul, W.W. Müller et J.Ryckmans, l'a maintenue dans le Sabaic Dictionary ..., Louvain-la-Neuve, 1982, aux pp. 29, 47 et 106. Seul le lecteur informé sait que Robin a narqué Beeston et ses suppôts, mais le lecteur ordinaire n'y verra que du feu; et ceux des compères qui restent sur la scène publique se garderont bien de relever le gant afin d'éviter de s'attirer des ennuis, de refroidir l'athmosphère ou de paraître irritables.Robin a certes le droit de tenir pour "inintelligible" l'inscription de son choix, mais il ne l'a pas de passer sous silence tout essai d'interprétation pas plus que son rejet antérieur par un tiers; cf. déjà Orientalia, 48 (1979), p. 495: "yet another imaginative interpretation of RÉS 2752", petite phrase qui pose la question de plagiarisme de la part de Robin.

Voici quelques courts exemples pris au hasard du sondage.

a - Les pronoms désignant des noms divins. — Mon livre Sabaean Inscriptions from Maḥram Bilqîs (Mârib), Baltimore, 1962, a introduit l'initiative de transcrire en majuscule l'initiale de tout pronom personnel ou adjectif possessif désignant une divinité pour le distinguer immédiatement de ses semblables se rapportant à des humains. Ce petit sytème bien simple a été généralement adopté, et Robin ne fait pas exception. Cependant, comme il est "Robin", il se devait à lui-même de compliquer la situation. Il limite le système uniquement au pronom personnel: on trouve "Il" dans 7 passages (pp. 72, 75, 98, 111 et 126) dominant de toute leur hauteur le solitaire "il" (p. 104). Quant au pronom personnel objet, on trouve les 2 orthographes à peu près en nombre égal: "Lui" dans 3 textes (pp. 98, 106 et 126) et "lui" dans 2 (pp. 75 et 175). Mais, l'adjectif possessif "son" ou "sa" n'est pas soumis à la nouvelle orthographe (pp. 99 et 75 respectivement).

b - C'est faire preuve d'un snobisme digne des "Précieuses Ridicules" que de rendre le pronom relatif d introduisant un nom propre par "dhū-"; p.ex., "ᶜmdrᵓ ddd ᶜAmmīdharaᵓ dhū-Dud" (Haram 2/1-2, pp. 62-63; à remarquer le trait d'union précédant "Dud"). L'initiative de Robin n'est que la conséquence de son pan-arabisme classique aussi aveugle que ridicule (cf. déjà mes Miscellanées ..., X [1979], p. 27). Les auteurs de travaux en arabe classique ne comprenant pas la signification réelle de d l'ont simplement intégré dans le nom lui-même; c'est ce que Robin copie servilement, comme le prouve le trait d'union devant le nom ethnique. Mais, quand d introduit un substantif, la boussole de Robin débranchée de son aimant pan-arabique classique est détraquée, et le d devient un simple "de". La péricope de Haram 2/1-2 (cf. plus haut) est immédiatement suivie par "dᵓhl" (l. 2) que Robin traduit par "du clan de", traduction incomplète, car la valeur spécifique de d n'est nullement rendue, le simple "de" français introduisant le terme en annexion.

c - Un individu offre au dieu Mutîbnaṭyân hwrn/mtᶜt (Haram 2/5-6) que Robin traduit par "la hiérodule (?) Matᶜat" (p. 63). Aucune étymologie de hwr n'est recherchée (cf. déjà plus haut, p. 47, sur l'absence de preuve dans les argumentations de Robin). Toutefois, maniant la flatterie quand elle lui paraît profitable ou opportune, Robin retient comme traduction de hwr le terme d'hiérodule suggéré par A.F.L. Beeston dans BiOr, 10 (1053), p. 201A. C'est du simplisme aussi bien "Beestonien" que "Robinien" que de croire que toute personne offerte à un dieu dans son temple ne pouvait être qu'un ou une hiérodule. De plus, le nom masculin mtᶜt, comme dans les ḥaḍramoutiques Ja 937/1 et 987/1, que la légèreté de Robin a métamorphosé en nom féminin et que la girouette de Beeston a réduit à un humain asexué, peut enfin reprendre sa place normale. Pour le terme hwr lui-même, cf. l'arabe hâra (o), 1ère et 5e formes "être écroulé", hâᵓir "abaissé" et le yéménite mihtar "ignoble, esclave". Il s'agit, donc, d'une personne de basse condition, quelle que soit d'ailleurs la nature de sa prestation dans le temple. Il convient, cependant, de faire remarquer que l'Arabie du Sud antique ne connaissait pas le système moyenâgeux de "classe" auquel Beeston ne cesse de faire appel. Une dernière remarque montre à quel point Robin se gobe. Il déclare que "bien que le toponyme Haram soit tombé aujourd'hui en désuétude, nous le retiendrons pour désigner le site, puisqu'il correspond exactement au nom antique" (p. 11). Comment est-il possible de se leurrer à ce point, de s'autosuggestionner et de dérailler au point d'affirmer que le "nom antique" de hrm(m) était "Haram" et non "la prononciation correcte ... Harim" selon A.Fakhry; cf. An Archaeological Journey ..., Cairo, 1952, I, p. 143, note 1:

> "Some of the inhabitants pronounce it 'Haram', others pronounce it 'Heram', but many have assured me that its proper prnounciation [sic] is 'Harim'."

Quel malheur qu'il soit impossible de contacter le djinn de Robin pour savoir comment ce dernier a appris la prononciation "antique" du nom!

d - Dans al-Ḥarâsif 2 (pp.199-200), Robin gaspille les 19 lignes de son commentaire, c.-à-d. 19 des 34 lignes de la p.200, à des conjectures insensées, comme celle de créer une phrase dans laquelle le second sujet est isolé du premier et rejeté en fin d'inscription après le complément direct du verbe. Robin n'a même pas remarqué ce qui crève les yeux, c.-à-d. le verbe bny introduit les noms des bénéficiaires de la construction. Il ne pouvait cependant pas le voir à cause de son identification hypothétique de l'auteur du texte, nbṯᶜly/ᵓmr/bn/ᵓlsmᶜ (aussi dans Kamna 17, pp. 188-189) avec le roi nbṯᶜly/mlk/kmnhw de RÉS 3945/17 (p. 159), un genre d'identification "à la Pirenne" qui ne mérite aucune considération, mais qui est le genre de raisonnement auquel Robin a sans cesse recours; en d'autres termes, il lui suffit de trouver un nbṯᶜly à Kamna ou ailleurs (p.ex., à al-Ḥarâsif 2/1, pp. 199-200) et l'identification avec le roi de RÉS 3945/17 est affirmée, même dans le cas d'absence de photo du texte.

En outre, à propos de la l.2, Robin parle de "deux angles droits qui créent l'illusion d'un encadrement" (p. 199). Même "l'illusion d'un encadrement" est pure illusion, un mirage trompeur puisqu'il n'y a même pas un semblant d'"encadrement", le cadre étant par définition une "bordure entourant" un objet. La moitié verticale de chaque angle a la hauteur des lettres de la l.2, et sa moitié horizontale est au niveau (et nullement au-dessous) de la ligne de base des lettres. Il s'agit donc d'un stratagème décoratif ingénieux consistant à allonger quelque peu la l.2 pour lui donner l'apparence d'être le piédestal de la l.1. En outre, le graveur a voulu isoler la carte d'identité de l'auteur dans la l.1: elle compte 16 lettres + 3 séparateurs de mots alors que la

l.2 n'en compte que 12 + 2. Si le graveur avait désiré rédiger son texte à l'ordinaire, c.-à-d. en le divisant en 2 lignes égales ou quasi égales, il aurait pu facilement donner 14 lettres à chaque ligne, mais il aurait dû forcément scinder en deux le nom du père de l'auteur.

Enfin, les 4 dernières lignes du commentaire que Robin consacre au texte Vérité 1/3 suggèrent une correction fautive de la lecture de J. Pirenne: le trait horizontal reliant les extrémités supérieures des 2 barres du b de š^{c}b est bien visible sur la photo.

Le texte d'al-Ḥarâšif 2, très clair sur la photo (pl. 59a) se lit comme suit:

1 nbṭ^{c}ly/^{ɔ}mr/bn/^{ɔ}lsm^{c}
2 angle droit bny/ḥmrw/wš^{c}bs angle droit

1 angle Nabaṭ^{c}alay ^{ɔ}Amir, fils de ^{ɔ}Ilsâmi^{c},
2 droit a construit [ceci] pour Ḥamraw et sa tribu. angle droit

e - al-Ḥarâšif 1 (pp. 199 et pl. 57b) est présenté comme un "petit fragment (de table à libations?)" de 6 x 16 cm; voilà toute la description muséographique que Robin a daigné jeter en pâture au lecteur. Quelle est la nature de cette pierre? Les pierres de la période sud-arabe vues sur le site ne sont pas davantage identifiées à la p. 197. Les dimensions données par Robin sont celles de la face antérieure trapézoïdale avec le coin inférieur droit brisé. De plus, cette même photo ne confirme pas la description de Robin puisque, pour autant que la photo permette de le voir, l'arrière du bloc est nettement plus haut que l'avant. Ensuite, et toujours d'après Robin, le texte "pourrait être complet à droite". Le côté droit du bloc paraît dégrossi sans toutefois être poli. Le caractère douteux de ses notes rend plus aiguë encore la déception de trouver que la photo du fragment est nettement disproportionnée, comme l'est déjà d'ailleurs celle de la pl. 2a, en raison de l'espace minuscule donné à l'avant du fragment (1 x 2,6 cm) comparé à la totalité de la photo (12,9 x 8,5 cm)! Que d'espace perdu, sacrifié à la foucade de Robin! Toujours d'après Robin, "la hauteur des lettres [ ] irrégulière [ ] peut être un indice d'archaïsme". Ce jugement est faux. Normalement, pareille irrégularité témoigne de l'amateurisme ou du dilettantisme de l'ouvrier graveur. Enfin, "^{ɔ}bk..." est complété en "^{ɔ}bk[rb" par Robin sans le moindre commentaire, comme si les noms ^{ɔ}bkhl, ^{ɔ}bkyn, etc., n'étaient ni aussi probables ni aussi possibles. Plus étonnant et plus grave encore est le fait que Robin ne se pose même pas la question du côté gauche qui est apparemment tenu pour certainement fragmentaire puisque le texte "pourrait être complet à droite" et que, par une décision oraculaire de Robin, il s'agit d'un "petit fragment." Il est donc nécessaire que le côté gauche original soit absent, quelle qu'en soit d'ailleurs la raison, brisure ou coupure.

Toutefois, l'arête latérale gauche est bien droite; il ne s'agit donc pas d'une cassure. En outre, ce qui reste de l'arête droite originale montre que la surface antérieure était trapézoïdale. Enfin, l'intervalle entre ^{ɔ} et b et entre b et k a respectivement 1.15 et 1.1 cm de largeur sur mon agrandissement. Par contre, l'intervalle entre le centre horizontal de k et le bord gauche est large de 1.25 cm, et l'intervalle entre le bas de k et le bord gauche est 1,4 cm. Ces mesures excluent donc normalement la possibilité de lettre à la gauche de k; et Robin n'a pas le droit de suggérer que ce cas serait ou même pourrait être une exception. Donc, la pierre n'a été ni cassée ni recoupée à gauche. Pour le nom ^{ɔ}bk, cf. bk (= bwk) comme verbe en minéen Hu 241a et en qatabanite Ja 1892 A/1, et aussi comme nom personnel et verbe en thamoudéen (cf. mes Miscellanées ..., V [1974], p. 130A).

f - La pierre tombale de 2 femmes, CIH 715, se termine par hgrytnhn (1.3; cf. pl. 8b) que Robin traduit par "de Hagar" (p. 94). Comment comprendre que quelqu'un puisse imaginer et surtout préférer une traduction aussi vague qui ne peut que le forcer à expliciter dans un commentaire ce qui aurait dû se trouver de toute nécessité dans sa traduction, savoir "les deux Hagariennes"? Cette traduction précise, complète et claire, rend tout commentaire superflu pour la forme elle-même. Mais Robin va plus loin: il désire que l'adjectif d'appartenance, la nisba, soit "probablement formée sur Hgr". Ici encore, Robin n'explique pas son affirmation. Son "probablement" ne s'explique pas, car les 3 lettres de hgr doivent de toute nécessité se retrouver dans le nom dont l'adjectif est formé; donc, hgr ou hgrm ou hgrn ou une autre dérivation. Le nom de lieu (pas nécessairement de ville) hgr se retrouve dans Gl 1265/1 = 986/1 (cf. K. Mlaker, Die Hierodulenlisten von Ma^{c}în, Leipzig, 1943, p. 26) qui provient de la même région. Les lieux nordiques de provenance de toutes les femmes mentionnées dans ces listes sont à ce point majoritaires que hgr est lui aussi et presque certainement nordique. De plus, les lieux appartenant au nord et nord-ouest sont tellement nombreux qu'il est beaucoup plus probable que hgr soit lui aussi l'un d'eux. L'opinion de Robin selon laquelle ces 2 femmes "semblent originaires de l'oasis de Hagar (Hgr) en Arabie du nord-est" (p. 55; après la mention de von Wissmann 1975, corriger "13" en "11") n'est fondée ou favorisée ou recommandée par aucune donnée épigraphique ... et, ici encore, la seule raison que Robin puisse donner, si par impossible, il condescendait à s'expliquer (cf. plus haut, p.54), serait ... son imagination.

g - Le fragment CIH 509 (p. 91; sans photographie) se termine par by[... qui se complète tout naturellement en by[wm lors[que ou au te[mps de (cf., p.ex., CIH 513/6 et RÉS 2740/12) au lieu de l'invraisemblable by[t de Robin, qui ne fait d'ailleurs aucun commentaire! De plus, c'est un des cas où Robin

utilise le signe d'un V couché sur le flanc, < >, pour indiquer le début et la fin d'une restitution de texte, alors qu'il se servait encore de crochets, [ ], 2 pages avant (p. 89, dans RÉS 2749), comme déjà tout au début de son pamphlet, dans ʾInabbaʾ 3 (p. 6). Quelle peut bien être la différence entre les 2 systèmes? Devine qui pourra!

9 - Étude de 12 exemples courts.

a - Haram 1 (p. 61 et pl. 6b).

C'est le 1er texte du plus long chapitre (pp. 9-126) du pamphlet, un texte inédit, le plus court de toute la série, puisqu'il se limite à un nom de 3 lettres seulement. Robin n'a donc eu aucune aide extérieure pour l'introduire, le guider ou lui suggérer des idées pour son travail. Il se trouvait,on peut bien le dire,au pied du mur.Il s'agit donc d'une étude en tout point originale qui met à nu tout le processus de sa formation scientifique. Il ne s'agit pas ici d'échafauder des gratte-ciel bien ordonnés et farcis de développements historique, social et religieux, qui foisonnent dans les publications modernes. Il s'agit de publier objectivement un ancien objet tout en respectant l'oeuvre d'un professionnel, voire même d'un amateur. En outre, l'usage considérable de la paléographie de J. Pirenne montre bien la place exceptionnelle que ce volume occupe dans la formation de Robin; il était bien plus que son livre de chevet,quelle que soit d'ailleurs la raison, politique ou non, de son choix.

1 - La description de Robin contient un peu plus de 4 lignes.

(a) - Robin N'informe PAS le lecteur des points suivants, c.-à-d.

(1) la nature de la pierre;

(2) la surface est dégrossie, mais non polie;

(3) les losenges des 2 yeux sont en relief au milieu d'évidements circulaires;

(4) l'axe vertical de l'oeuil droit est marqué par une ligne en creux;

(5) la forme de la stèle est irrégulière contrairement aux dimentions d'un parfait rectangle de 22,7 x 15 cm données par Robin; donc, proportion de 1,51, qui est celle des bords gauche et inférieur de sa photo, 7,9 x 5,2 cm. Mais, les côtés droit et supérieur mesurent respectivement 8,2 et 5,6 cm; donc, proportion de 1,46; et

(6) aucun détail sur la gravure du texte.

En conséquence de quoi Robin ne peut nier avoir bâclé son travail d'une manière on ne peut plus superficielle et cavalière, c.-à-d. "à la Robin".

(b) - La 1ère conséquence de cette désinvolture est étalée à la p. 35 où Robin déclare que "Ces documents, souvent frustes et maladroits, sont difficiles à distribuer chronologiquement. C'est pourquoi nous les traitons à part. "

Il est certes facile, mais combien injuste de traiter un travail de gravure de "frustre et maladroit" ... quand on n'est même pas capable soi-même de tracer des dessins, des

croquis convenables (cf., e.g., les croquis de Robin des graffites d'al-Ḥasâʾin; cf. plus haut, p. 47) et qu'on a recours à "tibi mei" pour suppléer à sa propre incurie. Évidemment, un "Directeur de recherche" a à sa disposition toute une ribambelle de dessinateurs de tout acabit, à qui il faut tout de même donner du travail, ne serait-ce que pour justifier leur présence sur la liste des salariés ; cf. aussi plus loin, pp. 73-74.

La description de Robin montre bien qu'il n'a même pas essayé de faire son travail d'épigraphiste. Mais, son mentor, M. Rodinson, n'étant pas un épigraphiste lui-même, n'a pas pu initier Robin aux nécessités et aux exigences particulières de cette discipline et, tout spécialement, au respect des graveurs et de la moindre ligne, indépendant des cadres fictifs qu'une simple introduction ou des connaissances générales peuvent faire germer dans le cerveau.

On ne peut qu'espérer que ce ne soit pas symptomatique d'une nouvelle ère que le pamphlet de Robin soit paru une année seulement après l'impression de l'ouvrage désastreux d'E. Bernand, A.J. Drewes et R. Schneider, Recueil des inscriptions de l'Éthiopie des périodes pré-axoumite et axoumite (Académie des Inscriptions et Belles-Lettres), Paris, 1991, 2 vol., dont mes Miscellanées ..., XVIII (1995), pp. 3-53, ont publié un compte rendu le vouant au pilon.

2 - Mon étude de Haram 1.

Schéma de minéen Haram 1.

Largeur de la stèle: 21,2.

Les chiffres pris sur mon large agrandissement sont en cm.
Proportions: 1/2 = 6x3 : hauteur/largeur de la lettre s,
à mi-chemin entre 1/2 et 1/3 = 6 x 2,5 dans
1- hauteur et section supérieure de la lettre s, et
2- hauteur/largeur des deux lettres b and l.

(a) - Si l'on prend la peine d'enregistrer les données de cette petite inscription, on se rend immédiatement compte de l'épure de la gravure. Afin de faciliter la prise des mesures, j'ai fait de la photo un agrandissement plus large que nature puisque la largeur de la stèle à la hauteur de la base des lettres est 21,2 cm alors qu'elle ne serait que de "15" (?) d'après Robin. L'exécution matérielle n'est malheureusement pas à la hauteur de l'épure dont l'ouvrier est sorti et qui prévoyait 2 séries de lignes fi-

nement tracées et destinées, on le sait, à déterminer l'emplacement des lettres dans les espaces ainsi délimités.

La 1ère série se compose de 3 horizontales, dont les 2 extrêmes encadrent les lettres, et la 3e divise la hauteur des lettres en 2 parties inégales, indiquant l'emplacement exact de tout trait horizontal. En fait, s est la seule lettre qui en possède un. La hauteur des lettres étant de 6 cm et la ligne mitoyenne à 2,5 cm de celle du dessus, la ligne du milieu passe donc à mi-hauteur entre 1/2 et 1/3.

La 2e série est faite de 4 traits verticaux, dont les 2 extrêmes déterminent les 2 marges latérales flanquant le nom propre, et le dessinateur de l'épure avait décidé que la largeur de la marge serait, elle aussi, de 2,5 cm. La 1ère inexactitude se voit dans la distance séparant le bord gauche de la stèle de la ligne verticale: elle n'est que de 2,1 cm au lieu de 2,5 cm. En outre, comme le montre le schéma de la page précédente, les 3 sections ont des largeurs qui vont en augmentant de droite à gauche, c.-à-d. 4,7, 5,8 et 6,1 cm; et celle du centre a presque la bonne largeur. De plus, aucune des 3 lettres n'est centrée dans le rectangle assigné. Toutefois, le graveur a manifesté d'une manière remarquable qu'il était à cheval sur les proportions 1/2 et un peu plus, avec toutefois une préférence pour la 2e. En effet, il maintient la proportion 1/2 dans la hauteur/largeur de s, mais il retient la proportion à mi-chemin entre 1/2 et 1/3 pour la hauteur/section supérieure de s et la hauteur/largeur des autres lettres, b et l.

Le témoignage de l'épure indique que le graveur appartient à une période de transition où la proportion 1/2 cède du terrain à un style moins alourdi et plus élancé; cette période de transition pourrait être placée vers les 10e-9e siècles avant notre ère. Les insuffisances du graveur, quelle qu'en soit la cause, n'ont pas diminué son témoignage qui reste certain et clair.

(b) - Étymologie.

La lecture du nom ne fait aucune difficulté, sbl, comme dans l'édition. Mais, Robin a à peine fini de transcrire le nom qu'il ne peut maîtriser plus longtemps son panarabisme, et il s'en donne à coeur joie: le minéen sbl est vocalisé ... "Subbul"!?!? La raison qu'il offre de sa vocalisation est révélatrice de son fanatisme myope tout autant que ridicule: " Commentaire : comparer peut-être avec l'arabe *Sunbul*, un nom de famille à Ṣan'ā', en supposant une assimilation du *n* dans *S'bl*. "

Il est bien regrettable que cette argumentation force le lecteur à se poser la question de la santé cérébrale de Robin. Ce n'est certes pas un brevet d'intelligence que chercher midi à quatorze heures. Et pourtant, qui sait?; c'est peut-être là le meilleur de l'enseignement dialectique reçu de son mentor.

1 - Le lecteur n'est nullement dupe des roueries, même littéraires, de Robin. Son "peut-être" et son "en supposant" ne sont que du poivre aux yeux de la galerie. Robin lui-même n'y croit pas; autrement, il n'aurait pas manqué d'ajouter un point d'interrogation après sa vocalisation de "Subbul".

2 - Il n'est pas suffisamment honnête pour informer le lecteur de l'existence de l'arabe sabal "pluie" et sabīl (pl. subul) "chemin, route" (cf. son dictionnaire de chevet, celui d'A. de Biberstein-Kazimirski, Paris, 1860, I, pp. 1047-1048). Ces mots et leurs dérivés stigmatisent tous l'apriorisme et la futilité de sa position.

3 - La valeur probante du nom moderne yéménite, qui n'est nullement soumis aux fluctuations des relations sociales et politiques de Robin, se limite exclusivement à confirmer-ce qui est de toute évidence superflu - l'existence d'une racine arabe snbl, qui n'est ignorée de personne. Donc, de ce point de vue, Robin connaît le succès mirobolant et facile ... d'enfoncer une porte ouverte.

4 - Robin continue à narguer le lecteur et pousse sa générosité jusqu'à lui jeter en pâture 2 os à ronger, c.-à-d. 2 noms arabes. Ils sont introduits par "Voir aussi", ce qui indique que ces noms sont compris dans la perspective de la phrase précédente qui est reproduite ici-même, p. 62, savoir comme des dérivés de la racine snbl. Semblable procédé d'argumentation est injurieux pour le lecteur.

a - Le 1er nom, en effet, "Abū Sunbula", confirme bien l'existence de la racine snbl, mais le nom sud-arabe est ... sbl (et nullement snbl et moins encore ʾbsnblt); donc la mention de ce nom est injustifiable. — Le 2e, "Sabalān", est emprunté à la p. 491B d'un ouvrage de W. Caskel. Mais, Robin ne fournit aucune preuve que ce nom dérive de la racine snbl, et c'est volontairement que Robin cache au lecteur l'existence de la racine sbl qui, d'une part, explique parfaitement le nom sud-arabe sbl, et d'autre part est bel et bien mise au rancard ou, peut-être pour plus de sûreté, dans le dépotoir de d'Académie par Robin qui mélange tout pour pouvoir mieux tromper le lecteur.

b - Robin aurait pu(?) aussi suggérer que les dérivés onomastiques des 2 racines sont en nombre égal. Cette question est sans importance pour justifier une étymologie. Toutefois, Robin se garde bien de soumettre au lecteur l'information nécessaire, et les détails suivants prouvent sa volonté de reléguer dans ce fameux débarras académique une autre information qu'il préfère taire, savoir les dérivés onomastiques arabes de la racine sbl sont nombreux.

c - Le volume de W. Caskel, dont Robin a extrait son 2e nom arabe, fournit aussi à la p. 514B, les noms masculins "Subaila" et "Subāla". Et il y en a encore d'autres, p.ex., "ʾIsbīl" de F. Wüstenfeld, Das geographische Wörterbuch des ... el-Bekrí, Osnabrück, 1976, pp. 82 and 122); and "Sabalu", "Sablalu", "Sabalânu", "Subullatu" et "Subullâtu" de F. Wüstenfeld, Jacut's geographisches Wörterbuch, Leipzig, III, 1868, pp. 34 et 35. Pourquoi donc Robin n'a-t-il pas le courage de les signaler au lecteur ou, tout au moins, de l'avertir de leur existence?

5 - Ce qui, toutefois, est le plus outrageant dans le commentaire de Robin, c'est la manière honteuse dont il traite la partie épigraphique sud-arabe, la seule qui, après tout, compte réellement. Une fois de plus, la généreuse pitance réservée par Robin au lecteur se compose, ici aussi, de 2 os à ronger, c.-à-d. RÉS 3945/18 et ẒM 206.

a - Dans le sabéen RÉS 3945, le nom sbl (1.18) est dit par Robin être "un nom de tribu ou de région". L'expression étant ywm/mhḍ/sbl, le sens de "région" est exclu par mhḍ "défaire, vaincre", qui ne peut s'appliquer qu'à des personnes, nullement à une parcelle de terrain, quelles qu'en soient d'ailleurs les dimensions.

b - ẒM 206 a été publié par J.Pirenne dans CIASA, I, pp. 481-484, où il est donné erronément comme "Ẓafār 56", sous la cote en accordéon "Pirennéen" on ne peut plus répugnante de ...
la version "Robinienne" en reproduit "R 82/s8/43.11, n° 2" (p. 481);
erronement le début en..... "R 48" (au lieu de "R 82"); et
le dernier élément est reproduit tout
aussi erronement dans "Tables" du
CIASA, II, 1986, p. 89, en ..... "39.11" (au lieu de "43.11").

Il convient de noter, cependant, que la cote est bien reproduite dans "Tables" du CIASA, I, 1977, p. 45. C'est donc Catherine Fauveaud-Brassaud, la réalisatrice des "Tables", II, qui est responsable de l'erreur nonobstant l'appareil high-tech du "Laboratoire d'Informatique pour les Sciences de l'Homme (L.I.S.H.) du Centre National de la Recherche Scientifique".

Le nom zodiacal sbltn qui orne le flanc gauche de cet élément architectural est bien, en fait, l'argument de base dans la diatribe de Robin, l'information qui, selon lui, doit de toute nécessité convaincre le lecteur du bien-fondé de son "assimilation du n". C'est une lapalissade évidente que de dire que, mises à part les 3 lettres sbl, ẒM 206 a autant en commun avec Haram 1 que l'astrologie avec un cimetière.

c - Pourquoi donc Robin passe-t-il également sous silence en les remisant, eux aussi, dans son dépotoir académique les autres dérivations sud-arabes de la racine sbl? P.ex., le nom d'homme sabéen sblt de CIH 983, dont le commentaire s'en réfère déjà à l'arabe "dw sblt" d'Ibn Durayd; le nom de temple ʾawsânite sbln de RÉS 454/4 (cf. les idées saugrenues de J. Pirenne dans CIASA, I, pp. 231-232); et le nom d'homme sabéen sblly dans RÉS 3618 (le commentaire d'A.J. Drewes - R. Schneider dans Recueil..., p. 161, RIE 86, porte à faux; pour mon commentaire de l'ouvrage précédent, cf. plus haut, p. 61).

d - Pour le substantif de mauvaise lecture, "msbl" de RÉS 5055 B encore signalé par J.C. Biella, Dictionary of ... Sabaean Dialect, Chico, 1982, p. 326, cf.mon étude, que Biella ne mentionne même pas, dans Cahiers de Byrsa, 6 (1956), p. 175: il s'agit du nom personnel sʾ(1).

6 - On peut encore signaler le nom nord-arabique sbl en safaïtique, p.ex., Wi 545, et en thamoudéen, Eut 156.

b et c - RÉS 3328 et 3330 (respectivement, pp. 114-115 et pl. 14d et 17a).

Ces 2 études de Robin sont d'autres exemples d'un cynisme froid et calculateur, où il est fait fi de tout ce qui ne lui plaît pas pour satisfaire une fois de plus - que pourrait-il faire d'autre? - son égocentrisme insatiable. Elles ne méritent pas qu'on s'y attarde. La reproduction de mes 2 études dans mes Miscellanées ..., II (1971), p.62, maintenues ici nonobstant les rêveries du couple Pirenne-Robin, est suivie de quelques remarques appropriées.

"RÉS 3328. - Stèle funéraire en calcaire jaunâtre; épaisseur constante: 5,5 cm. - Face antérieure: 15,8 (haut) et 16,1 (bas) x 28,8 cm. Chaque oeil est représenté par deux circonférences concentriques, comme dans CIH 853. Ces dessins et le texte (9,7 x 3,4 cm.) sont gravés avec très peu de soin.

btʿn/ Batʿân.

E.g., Gl 1642/2. Les traits qui pourraient faire lire d au lieu de b sont superficiels et n'appartiennent pas à la lettre. Il y en a d'ailleurs d'autres et le plus marquant d'entre eux est une ligne brisée recourbée qui donne au n final l'apparence d'un monogramme composé d'un b sans hampe inférieure et surmonté d'un n.

RÉS 3330. - Stèle funéraire en calcaire jaunâtre, du même genre que celle de RÉS 3328, mais avec un cercle pour chaque oeil. - Epaisseur maximum: 8,5 cm. - Face antérieure: 11,1 (haut) et 16,9 (bas) x 35,7 (droite) et 36,2 (gauche). - La gravure du texte est très peu soignée. L.1: 11,7 x 3,3 cm.; interligne: 1,2 (droite) et 3,3 cm. (gauche); l.2: 11 x 3 cm.

klbm 1 Kalbum
bhr 2 Bahr.

bhr, cf. le nom personnel ʾbhr dans CIH 562 bis/2; CIH (II, p. 378) tient le mot pour un substantif et cette opinion est reprise par A.G. Lundin (cf. Vestnik drevniev istorii, 3 [1962], p. 114, no. 11)."

RÉS 3328. - Le nom se lit btʿn/ Batʿân. Les autres traits sont tellement superficiels et hybrides qu'il faut être anormal ou avoir des yeux atrophiés pour y voir des lettres. Dans son CIASA, I (1977), p. 548, Pirenne retient aussi "dtʿn" (donc, Robin est en bonne compagnie). Du moins, a-t-elle le courage de remarquer qu'il s'agit d'un "nom bizarre" ... que Robin doit trouver parfaitement normal: puisqu'il n'en dit rien, c'est qu'il le trouve bien régulier. De plus, G.L. Harding (cf. An Index [cf. plus haut, p.4], p. 381) parle d'une "root unknown". Cette information, Robin s'en fiche pas mal, convaincu qu'il est que la nouvelle idole de la cella académique n'en recevra pas moins les encensements quotidiens prescrits aux robots de l'endroit par le rituel sacro-saint des us et coutumes.

RÉS 3330. - Ici aussi, le couple Robin-Pirenne jouit encore d'une lune de miel bien éphémère, les 2 partenaires ayant la même répugnance pour les tracés en let-

tres cursives de graffite, où ils ont toujours brillé par leur insouciance, leur désintéressement et, conséquemment, leur incompétence.

d - RÉS 4674 = Hamburgisches Museum für Völkerkunde und Geschichte, = Hamb[g] 31.100:1607 = R[athjens] 83 (pp. 105-106 et pl. 10b).

Il convient de remarquer en tout premier lieu que Robin, selon son habitude, reprend des auteurs ce qui lui plaît et relègue dans son dépotoir ce qui lui déplaît. Mais, comment comprendre la bizarrerie de loup-garrou qui refuse au lecteur les données muséographiques publiées par les éditeurs, E. Mittwoch et H. Schlobies dans Orientalia, 6 (1937), pp. 85-86, comme, p.ex., "weissgelber Marmor. 10 x 9 cm" (p. 86), dont le RÉS (VII [1950], p. 310) n'a repris que la 1ère moitié?

L'étude de Robin accumule 5 erreurs grossières qui, on n'en peut douter, sont les conséquences de délibérations bien pesées et longuement ruminées.

(1) Le résultat inéluctable de son chauvinisme déjà mentionné est que le lecteur est dans l'impossibilité de se figurer les dimensions de l'objet et surtout de savoir que la photo de la pl. 10b est plus grande que nature. Qui pourrait le deviner? Les données suivantes et mon estampage furent pris au musée lui-même le mercredi 8 août 1956:

> bloc en albâtre blanchâtre; coins abîmés; épaisseur constante à la l.1: 6,9 cm; largeur: 7,7 (centre de la l.1), 8,2 et 8 (centre des ll. 2 et 3) et 7,3 (haut de la l. 4); hauteur des lettres: 1,4, 1,5 et 1,4 pour les ll. 1-3, le bas de la l.4 manque; interligne: 0,3 cm de même parfois 0,25, p.ex., sous le trait de séparation des mots apres ʿbd de l.1.

L'espace libre au-dessus de la l.1 étant large de 0,5 à 0,7 cm démontre que l'interprétation des éditeurs, reprise par RÉS (cf. l.c.,) et par Robin, selon laquelle une ligne supérieure est restituée, est manifestement erronée. Il est bien certain que les mesures des éditeurs, "10 x 9 cm," sont exagérées. Même à Hambourg, je ne suis pas parvenu à m'expliquer ces chiffres, même en ajoutant à la surface intacte les rebords latéraux endommagés.

(2) La 2e grosse erreur de Robin, également copiée du RÉS qui l'avait reprise des éditeurs, est la restitution de "[dt]" (ll. 3-4) avec antécédent ġlym. Mais, le commentaire de G. Ryckmans fait suivre le "[dt]" d'un point d'interrogation, comme le note E. Littmann dans son compte rendu de RÉS, VII (cf. ZDMG, 101 [1951], p. 375): ġlym ne peut être féminin puisque l'auteur est masculin; en foi de quoi J.C. Biella lit "ġlymn [d]ṯfthw" (cf. Dictionary ..., p. 425). Hélas! pour le grand seigneur Robin, G. Ryckmans, Littmann et Biella ne représentent rien qui puisse l'influencer. Comme on aurait dit de Robin dans le temps à Louvain: "Il se trompe avec la [sic] sexe".

(3) La 3e erreur de Robin est d'avoir arrangé les lignes de son mieux en leur donnant une apparance de balancement qui est contredit par la photo; p.ex., le h de 1.2 est en fait dans l'axe de la moitié gauche de t de 1.1; dans son chef d'oeuvre architectural, il est 3 lettres à droite! Son truc était, évidemment, de leurrer son lecteur pour influencer favorablement sa décision. En fait, il a construit un monstre puisque, selon la p.106, le nombre de sa fausse l.1 ne correspond à celui d'aucune ligne et serait d'ailleurs incompréhensible étant donné qu'il rejette la restitution d'un "bn" suggérée par les éditeurs et acceptée par RÉS. En outre, aucune lettre n'est à sa place. Somme toute, le schéma de son déchiffrement s'établit comme suit: sa l.1: 12 + 3; sa l.2: 11 + 2; sa l.3: 10+? + 2; sa l.4: 11 + 3; et sa l.5: 14 + 2.

Voilà un schéma bien digne de J. Pirenne!

(4) La 4e erreur de Robin est de créer ou d'éliminer des traces de signe pour s'adapter à ses formules préconçues.

(a) Comme l'indiquent déjà les éditeurs (suivis par RÉS), il reste encore une partie du trait de séparation des mots avant hqny de 1.2, ce qui normalement suppose une lettre à droite. Robin n'en tient aucune compte.

(b) À l'instar des éditeurs et du RÉS, il restitue d à la fin de la l.3 alors que la pierre porte 2 petits traits verticaux rapprochés l'un de l'autre dans la partie supérieure de la fin de la ligne; donc, ⌊h⌋.

(c) Comme dans le cas précédent, Robin élimine un petit trait vertical dans la partie supérieure du début de 1.4, qui appartient à un n; donc, (h)(4)[g]⌊n⌋, comme l'indique déjà mon cahier de notes prises à Hambourg.

(5) La 5e erreur de Robin est de démentir une traduction clairement définie plus haut dans son pamphlet. À la p. 55, en effet, Robin prétend que le terme ʿbd/(pl.) ʾdm "montre la qualité de 'client(s)'" dans CIH 534/2 et RÉS 4133/6. Dans le présent texte, il traduit ʿbd par "serviteur" comme si "serviteur" et "client" étaient des synonymes parfaitement interchangeables. Pour ʿbd, cf. ici, p. 55 et note à p. 98. Il faudrait tout de même que Robin publie un dictionnaire de son jargon. La traduction erronée "client" n'a de consistance que la conception vaporeuse moyenâgeuse dont A.F.L. Beeston et sa clique endossent la société sud-arabe (cf. aussi plus haut, p. 57, et plus loin, pp. 79, etc).

Le texte de RÉS 4674 s'établit donc comme suit:

[ʿ]tt/ʿbd/dšr q[r]	1	[ʿA]tat, serviteur (du clan) de Šaq[r-]
[n]⌊/hqny/dsmwy/⌊b⌋	2	[ân], a dédié à Dû-Samâwî, m[aî-]
[ʿ]l/byn/ġlymn⌊/ḥ]	3	tre de Bayyin, cette petite statue, (co)[m-]
[g]⌊n/ṯfth⌋ w⌊/lh⌋[w]	4	[m]e il la Lui avait promise.

Donc, l'alignement des signes sud-arabes est parfait dans son parallélisme:
1.1: 10 + 2; 1.3: 11 + 3;
1.2: 11 + 3; 1.4: 10 + 2. Note also that the man ʿtt of Ja 844/5 also is a dependent.

La paléographie est des plus intéressantes et unique en son genre, et Robin n'en souffle mot! La caractéristique la plus importante de cette gravure est la liberté entière dont le graveur fait état.

a - S'il a tracé 2 lignes finement marquées pour déterminer la hauteur des lettres, qu'il se plaît d'ailleurs souvent (p.ex., b de la l.3) à ignorer, il se garda de tracer une médiane ainsi que les verticales complémentant l'encadrement des lettres, et ces omissions le dispensèrent d'être assujetti à une quelconque rigidité dans la forme des lettres.

b - Les empattements sont toujours très discrets et parfois même inexistants (p.ex., b de l.1, hampes droite et gauche).

c - Les traits rectilignes (p.ex., les 4 traits du premier h de l.4) peuvent faire place à une légère courbe concave (p.ex., b de l.1, et haut de m de l.3), qui peut même être anguleuse (p.ex., h de l.2, section supérieure). Donc, il y a deux types de h et de m.

d - Les traits soit obliques dans la section gauche des m et l, soit horizontaux des s et ş, ne se rattachent pas à l'extrémité de la hampe centrale ou latérale, mais à une très courte distance de cette extrémité, ce qui peut donner l'impression d'une courbe (p.ex., bas de m et centre de ş de l.3).

e - L'interligne mesure 0.8 cm sur mon agrandissement.

La situation particulière de cette gravure polyvalente est nettement visible dans les mesures de chaque lettre prises sur mon agrandissement où les lettres sont hautes de 7 cm; cette liste est suivie du schéma des mesures échelonnées sur une grille de 7 cm.

Largeur des lettres:

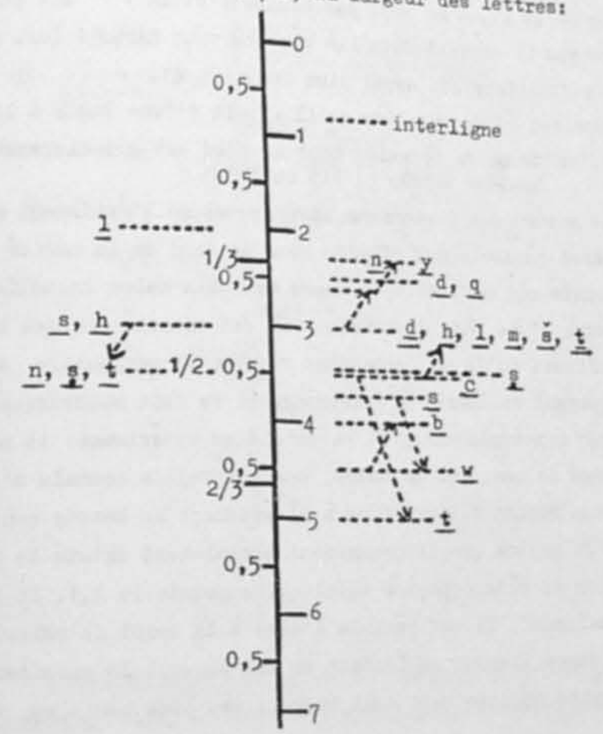
b: 4 et 4,5 cm (ll. 1 et 3);
d: 2,5 cm (l. 1);
d: 3 cm (ll. 1 et 2);
h: 3 et 3,5 cm (ll. 2 et 4);
w: 4,5 et 3,7 cm (ll. 2 et 4);
y: 2,3/2,7 et 2.7/3 cm (ll.2 et 3);
l: 3 cm (l. 3, deux fois);
m: 3 cm (ll. 2 et 3);
n: 2,3 cm (ll. 2 et 3, deux fois);
s: 3,7 cm (l. 2);
ç: 3,5 cm (l. 1);
ş: 3.3 cm (l. 3);
q: 2,5 cm (l. 2);
š: 3 cm (l. 1);
t: 5 et 3,5 cm (ll. 1 et 4);
ţ: 3 cm (l. 1).

Hauteur de la section supérieure des lettres:

h: 3 cm (ll. 2 et 4);
l: 2 cm (l. 3, deux fois);
m: 3,5 cm (ll. 2 et 3);
n: 3,5 cm (ll. 2 et 3).
s: 3 cm (l. 2);
ş: 3,5 cm (l. 3);
t: 3,5 et 3 cm (ll. 1 et 4);

Dans RÉS 4674,

proportion de la hauteur des lettres et la section supérieure des lettres; 1) l'interligne et 2) la largeur des lettres:



Le cas hybride de RÉS 4674 peut se résumer comme suit.

Onze lettres, d, d, h, y, l, m, ç, ş, q, š et t appartiennent à la proportion de 1/2 - 1/3 avec une poussée de 3 lettres, b, w et s, entre 1/2 et 2/3 et même une double pointe en deçà de 1/3 avec y et n, et une au-delà de 2/3 avec t. La proportion préférée précédente entre 1/2 et 1/3 reste également la grande favorite dans le rapport hauteur de la lettre - hauteur de la section supérieure avec une pointe en dessous de 1/3 avec l. De plus, les empattements s'esquissent à peine ou sont totalement absents. Enfin, plusieurs traits rectilignes sont remplacés par des courbes, parfois anguleuses.

Tout étant pratiquement encore à faire dans le domaine paléographique, tout ce qu'il nous est possible de dire est que ce type hybride pourrait appartenir aux environs du 8e siècle avant notre ère à une époque où la proportion 1/3, fermement établie, cherchait son chemin pour les empattements et n'avait cependant pas supplanté complètement

la proportion 1/2 à moins que ces réminiscences de 1/2 ne soient une initiative personnelle du graveur lui-même de même que les quelques hardiesses dans l'introduction des courbes en remplacement des rectilignes.

e - ʾInabbaʾ 4 (pp. 7-8 et pl. 4 a-c).

Les 4 photos de la pl. 4 ne sont pas claires, et la 1ère est particulièrement ratée; il en résulte que le travail détaillé réalisé pour Haram 1 (cf. plus haut, pp. 60-62) est des plus difficiles; cf. aussi plus haut, p. 41.

Robin commence son étude par dire qu'il s'agit d'"une table à libations" de 17 x 27 x 36 au-dessus d'un "tronc de pyramide dont le pied est grossièrement équarri ... hauteur totale [] 115 cm."

Robin ne donne aucune mesure des 2 sections inférieures, de l'évidement supérieur et pas davantage de la tranchée centrale qui affecte même le fond de la cavité supérieure. C'est cette particularité qui contredit la thèse de Robin selon laquelle "le dégorgeoir avait peut-être la forme d'une tête de taureau: il est encadré par des traits verticaux". C'est donc un aveu indirect qu'il ne reste plus rien de la gargouille, et les "traits verticaux" n'en présupposent nullement l'existence. Il se fait malheureusement que Robin ne donne aucun détail sur cet emplacement et sa condition extérieure: il ne dit même pas s'il en reste des signes de cassure! En outre, une gargouille normale n'exige qu'un seul creusement, celui de son rebord faisant face à l'évidement du bassin central. Dans le cas présent, la photo 4a montre que le creusement a également entamé le fond lui-même du bassin central au milieu du côté opposé à celui qui supporte la l.1. Il ne peut donc s'agir d'une "table à libations". Il est pénible d'être à la merci de bricoleurs qui croient savoir ce qu'ils font et se fichent du lecteur en lui servant la sempiternelle pitance de photos dont la mauvaise qualité rend tout travail des plus pénibles, voire même impossible.

La recherche qui souffre le plus de cet état pitoyable est la paléographie, et les détails qui suivent devraient être vérifiés sur l'original et corrigés si besoin en est. Voici les 2 séries de mesures, comme celles de RÉS 4674 (cf. plus haut, p.68).

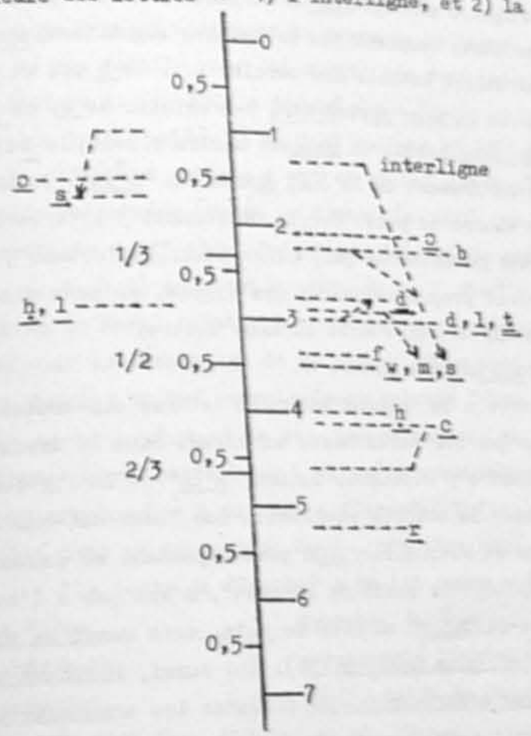
Largeur des lettres:

ʾ: 2,1 cm (l. 1, deux fois); b: 3,3 cm (l. 1); d: 3 cm (l. 2); d: 2,8 (l.1) et 3 cm (l. 3); w: 3,5 cm (l. 1); h: 4 cm (l. 2); l: 3 cm (l. 2).
m: 3,5 (l.2, deux fois) et 2,9 (l. 3); s: 1,3 (l.1), 2,5 (l.2, deux fois) et 2,3 cm (l.3); ʿ: 4,1 et 4,6 cm (l. 3); f: 3,4 cm (l. 3); r: 5,2 cm (l. 3); t: 3 cm (l. 2), et

Hauteur de la section supérieure des lettres:

ʾ: 1.5 cm (l. 1, deux fois); h: 2,9 cm (l. 2); l: 2,9 cm (l. 2) et s: 1 (l. 1) et 1,7 cm (l. 3).

Dans ʿInabbaʿ 4, proportion de la hauteur des lettres et la section supérieure des lettres | 1) l'interligne, et 2) la largeur des lettres:



Les mesures des ll. 2 et 3 ont été ajustées sur la hauteur de la l.1 de mon agrandissement, 7 cm. La hauteur des 3 lignes, en effet, est progressivement décroissante. Selon Robin, la hauteur est de 8 et 7 cm aux lignes 1 et 2. Il faut bien admettre que l'effort de donner cette double information a essoufflé Robin au point qu'il ne mentionne même pas la l.3.

ʾInabbaʾ 4 présente un autre genre d'hybridisme axé sur le canon de 1/2 - 1/3 puisqu'il montre 8 lettres dans ce centre, d, d, w, l, m, s, f et t, avec 2 autres, h et ʿ, entre 1/2 et 2/3, et ces 2 groupes sont flanqués de part et d'autre par un extrême au-delà de 2/3, r, et 2 autres en deçà de 1/3, ʾ et b.

Pour la proportion hauteur des lettres - hauteur de la section supérieure, 4 lettres se partagent également le terrain: en deçà de 1/3, ʾ et s et au-delà de 1/3, h et l. La différence entre les 2 textes réside dans le plus grand nombre attestant la proportion 1/2 - 2/3 au-delà de 2/3 dans ʾInabbaʾ 4 avec 6 lettres, w, h, m, s, ʿ et r, comparées à 5 dans RÉS 4674; b, w, s, s et ʿ; il s'agit donc d'une différence minimale. De plus, une seule lettre de chaque groupe va bien au-delà des 2/3, aux alentours de

5/7; respectivement r et t.

Par contre, la description paléographique de Robin pourrait être une pitrerie dans une comédie enfantine: "ce texte comprend peu de lettres significatives;" le "peu" étant précisé comme étant ... "la taille notable des cercles ..., le m ... et le r." Comme l'indique le tableau de la page précédente, les cercles de w, de t - il n'y a pas de y " , de même que les m font partie du peloton central; seul, le r fait bande à part.

Dans son déchiffrement de la l.2, Robin lit "m°lm". Comme on le sait, personne ne connait la valeur exacte du petit cercle couronnant 3 lettres de la l.2, pas plus d'ailleurs que celle des parenthèses qui, elles aussi, affectent 3 lettres de la l. 1. Quelle que soit la valeur propre de chacun des signes, on peut dire ceci:

a - ils ne se retrouvent jamais ensemble avec la même lettre;

b - ils peuvent cohabiter dans un même texte; et

c - le petit cercle est réservé à la transcription en lettres sud-arabes à l'exclusion de la traduction, tandis que les parenthèses survivent dans la traduction.

Comme il fallait s'y attendre, le mot "m°lm" de la l.2 fournit à Robin une belle occasion de ressasser la théorie vieillotte des "béni-oui-oui" de la bande d'A.F.L. Beeston, J. Ryckmans et W.W. Müller. On peut cependant se demander si la hantise de cette expression décrépite, "la salle du banquet", n'est pas à l'origine non seulement de cette lecture erronée de "m°lm" au lieu de mslm, mais aussi de son interprétation de "table à libations" (cf. plus haut, p. 70). Ici aussi, Robin se garde bien - par pure honnêteté professionnelle, assurément! - de discuter les arguments de ma dénonciation de cette hantise, ce prurit de voir partout des allusions ou références au culte.

Robin voit dans le début de la l.1 "d°b°ws", un nom dont "la lecture proposée est d'autant plus incertaine que l'élément °ws est d'ordinaire le premier membre des noms composés (et non le second)" (pp. 7-8). Et voilà bien un exemple typique de l'argumentation biaise et saugrenue de Robin.

1 - La 1ère affirmation est d'autant plus caduque qu'elle reviendrait à admettre pour principe de déchiffrement les connaissances du travailleur qui peuvent être fautives, comme c'est le cas ici.

2 - La dérivation - + °ws, bien que rare (cf. °l°ws du minéen RÉS 2742/1 et TaM 10/1 et sabéen, p.ex., Ja 831/1), est parfaitement normale, et le nouveau dieu de la cella académique parisienne erre en s'en servant pour jeter le doute sur cette lecture.

3 - Afin de pouvoir s'appesantir exclusivement sur l'élément °ws, Robin ne souffle même pas un seul mot sur le 1er élément, d°b, qui est bien attesté dans les noms theophores qatabanites ᶜmd°b (p.ex. Ja 1093/1) et ᶜmd°bm de Ja 1704.

4 - L'absence de tout examen paléographique digne de ce nom par Robin l'a aiguillé sur une voie de garage en le faisant passer à côté d'un fait qui aurait dû lui ouvrir les yeux, c.-à-d. le d initial est sensiblement plus large que les autres lettres de la même ligne (cf. le schéma de p.71), 2,8 pour d contre 2,1, 2,3 et 1,3 respectivement pour °, b et s; donc, d se détache nettement du reste de la ligne. D'ailleurs, Robin n'avait nullement besoin de ces détails pour lui ouvrir les yeux puisqu'il avait COPIÉ du CIH (II, p. 245) la lecture de CIH 530/1: d°lwhb = d + °lwhb, qu'il reproduit lui-même dans Haram 32/1 (cf. plus loin, p.76), là où d NE diffère EN RIEN des autres lettres! Si le cas de CIH 532 est différent puisque d est en relief sur le rebord de droite, Robin fait montre d'une myopie volontaire bornée en disant (p. 100) que, d'après moi, "la 1.10... commence à la verticale du d" et il "n'est pas très clair sur la photographie" que "les quatre t (ll. 4, 6, 6 et 7) seraient ouverts en bas". Toutes ces précisions sont évidentes sur la photo de la Royal Asiatic Society qu'il reproduit et dont j'ai une copie, pour quiconque désire voir le témoignage de la plaque. Pour sbdt, cf. plus loin, p. 82. En plus, Robin nous fournit un bel exemple de sa volonté ferme de faire ce qui lui plaît dans sa copie du texte nonobstant le témoignage certain de la photo: alors que les initiales des 3 lignes sont parfaitement alignées verticalement dans l'original, la copie de Robin les fait commencer à 3 niveaux différents! Enfin, pour la servilité pan-arabe classique de Robin dans sa traduction de d-, cf. plus haut, p. 56.

Le texte de °Inabba° 4 se lit comme suit:

d °b°ws	1	Symbole. °Ab°aws
sbdt/mslm	2	a renouvelé l'autel à encens de
smᶜ/dfrᶜ	3	Sâmiᶜ, Celui de Faraᶜ.

f - RÉS 3303 (pp. 119-120 et pl. 19 a-d).

L'habitude de Robin de mentionner les numéros de "M" du livre d'A. Capuzzi (1974) reste injustifiable, ce livret n'étant d'aucune utilité puisqu'il ne contient que les copies, fourmillant d'erreurs, de publications antérieures. Le mieux que le lecteur puisse faire, c'est de l'oublier, sinon il serait forcé par la décence et l'objectivité scientifiques d'en corriger les erreurs, ce qui est tabou pour Robin à cause du récent engoûment pour les petits frères du sud, que le djinn du Wâdî Yalâ, après un vol de presque 5500 km, a fait germer dans le coeur de la nouvelle idole de la cella académique et dans celui des grands frères du nord (cf. plus haut, pp. 37-38 et plus loin, pp. 93 et 208).

Robin s'en réfère aux photos de la "pl. 19 c-d" (p. 119), où le lecteur ne peut voir qu'imparfaitement 15 lettres des 23 du texte; 4 lettres dans chacune des 2 lignes supérieures sont invisibles sur les 2 photos, savoir mᶜ et n/ṯ (1.1) et r/° et bn (1.2). Robin se réfère aussi aux 2 "fac-similés Hélène David, pl. 19 a-b" (p. 119), qui ne sont que des minables approximations et, ce qui est plus grave, la section a et l'extrême droite de b qu'il représente ne sont même pas identiques. Robin renvoie aussi au fac-similé de l'édition de J.H. Mordtmann (1893), qui est encore moins fidèle que ceux de H. Da-

vid. Comment comprendre que, dans la ribambelle des dessinateurs de tout acabit dont Robin dispose (cf. plus haut, p. 61), il n'en ait pas trouvé un seul ou une seule qui soit capable d'exécuter des fac-similés convenables? Les fac-similés publiés pourraient peut-être être acceptés au niveau d'école gardienne. Finalement, Robin ne mentionne ni l'éditeur ni l'ouvrage où le publication de l'objet a été faite. La pièce a été publiée par E. Glaser, Mittheilungen ..., Prag, 1896, p. 76, comme l'indique J.H. Mordtmann lui-même et, après lui, le RÉS.

Robin répète la description du RÉS, mais après l'avoir arrangée à son gré et sans raison. Ce faisant, il réitère la liberté prise par G. Ryckmans d'arrondir les 7,7 cm de hauteur, donnés par Mordtmann (p. 57, n° 9), en "0 m 08" que Robin retranscrit en "8 cm". La lecture du pamphlet montre que l'exactitude des dimensions des objets ou des lignes n'est nullement le fort de Robin. Enfin, Robin parle d'"un trait vertical" marquant "où commence chacune des trois lignes". Mais la photo montre 2 lignes verticales parallèles et très rapprochées, et nullement "un" seul trait, et cette double ligne est reproduite très clairement sur le fac-similé b de la pl. 19. Finalement, cette double ligne est insérée entre le début des lignes et leur fin et de haut en bas.

Le symbole, qui introduit la l.1, ne fait l'objet d'aucune remarque (p. 120), même pas d'un renvoi à A. Grohmann, Göttersymbole ..., p. 66A, une publication qui ne figure même pas dans la bibliographie (p. 204) et qui résume J. H. Mordtmann et reproduit un fac-similé assez pauvre de tout le texte (avec un trait de séparation des mots en moins) dans la figure n° 171.

"Les triangles bombés et proéminents du m" de Robin (p. 120) n'existent que dans son imagination; toutes les lignes sont droites, et la largeur des triangles équivaut à peu près à 1/4 de la hauteur. "Les maladresses de la disposition", également au dire de Robin, existent dans les fac-similés de H. David, non sur les photos. De plus, les 2 fac-similés de H. David ne respectent pas la forme des lettres; p.ex., t de l.1 est nettement débanché sur son dessin et nullement sur la photo; l'angle central du m de l.2 touche à peine la barre verticale alors qu'il la superpose dans son dessin; les côtés latéraux de la cupule de b (l.1) sont des courbes dans les 2 dessins alors qu'ils sont des droites obliques se terminant par une large coupe dans le bas; cf. aussi plus haut, p. 71, pour la qualité des fac-similés. Enfin, la donnée la plus importante du commentaire de Robin est sa mention explicite de la lettre g. Fasciné par le texte de RÉS, Robin y a copié la transcription et la traduction, répétées aux pp. 44, 45 (pas de renvoi au précédent) et 52, qui toutes 2 remontent à E. Glaser. C'est cette fascination qui, très probablement, explique que Robin n'a même pas remarqué - est-ce possible? - que le fac-similé b, qu'il publie lui-même, montre la lettre g dont il a lui-même fait état dans sa "graphie" (p. 120; cf. plus haut) où il parle explicitement de la "barre du ♂ et du ♀".

..., et sa "copie" ne contient aucun g qu'il a copié tt? Mille fois hélas!, cette lettre se trouve sur une section du cylindre qu'aucune des 2 photos ne reproduit.

Le texte de RÉS 3303 peut se lire comme suit:

Br. drame	bmʿtt/tn/twr	1	Br. drame. Bamm'atat, fils de Tan-
	ktbr/ʾrš/mtbntyʿ	2	kabir, a payé l'amende pour une blessure à Mutibnatig-
	n	3	an.

Robin n'a pas un seul mot de commentaire, sûr, on peut en être certain, qu'il n'a rien à expliquer. Et pourquoi le ferait-il puisque ses dévots ne manqueront pas d'avaler ses décisions, quelles qu'elles soient ou puissent être, les yeux fermés dans une attente béate de la vérité parfaite (cf. déjà plus haut, p. 50). - bmʿtt: Robin transcrit "Bamʿatat", ce qui indique qu'il fait dériver la 1ère partie du nom théophore de la racine bmy. Il est en bonne compagnie puisque G.L. Harding fait de même (cf. An Index..., Toronto, 1971, pp. 202 et 203). Robin ne prend même pas la peine de prouver non seulement l'exactitude de son étymologie basée sur son pan-arabisme classique bien connu, mais aussi l'impossibilité d'en appeler à la racine bmm chauffer. - twrktbr/ʾrš (ll. 1-2). Comme twr de RÉS après Mordtmann et ce dernier après Glaser, Robin transcrit, lui aussi, twr en flagrante contradiction avec la mention de g dans sa graphie! (cf. déjà plus haut), qui va à l'encontre de la lecture d'E. Glaser et de J.H. Mordtmann qui, tous 2, ont t. On ne peut tout de même pas donner la préférence au travail d'une dessinatrice qui n'a pas été capable de reproduire correctement les lettres que le lecteur discerne sur les 2 photos. Voilà donc bien un autre exemple de l'embarras, des difficultés que la recherche connaît grâce au sans-gêne égoïste de Robin et l'incurie d'une dessinatrice soi-disant professionnelle, tous deux oeuvrant sous la mouvance et le patronage de l'Académie des Inscriptions et Belles-Lettres et du CNRS (cf. plus haut, pp. 73-74).

Toujours dans le sillon de Glaser ("Vorsteher der Priesterschaft") et de Mordtmann ("Oberster der Priester"), pour qui ʾrš est l'écriture défective du pluriel de rbm, reprise par RÉS, Robin traduit l'expression kbr/ʾrš par "grand prêtre" (pp. 44, 45 et 52) = "chef des prêtres" (p. 52), une interprétation également retenue par J.C. Biella, Dictionary ... (1982), p. 497, mais la soi-disant forme défective du pluriel ʾrš n'est pas mentionnée dans le Sabaic Dictionary ... sous rbw (p. 118; l'entrée "ʾrš" n'existe pas à la p.7), un témoignage qui n'a pas beaucoup de valeur, si tant est qu'il en ait même une. Personne n'a jamais prouvé que "ʾrš" était le pluriel de rbw, qui est ʾrbw dans CIH 41/1. Comme ʾrš est un hapax legomenon, on doit certainement et de prime abord rejeter cette interprétation car l'inscription ne la suggère pas. Por ʾrš, cf. l'arabe ʾaraša "payer l'amende pour une blessure" et ʾaršⁿ "prix du sang." - twrktbr est un nom

théophore composé, comme le sabéen twryt^{c} de Ph 122, et kbr est le 2e élément du nom théophore $^{\flat}$lkbr du qatabanite Ja 2393 et des sabéens Ja 2848 z/3 et ad/1. - mtbnṭyn (ll. 2-3); à corriger la coquille de p. 57: "Matbnaṭiyān" (sic).

g - CIH 530 (pp. 99-100 et pl. 10a).

Plusieurs informations données par Robin sont inexactes.

Pour le symbole initial, cf. plus haut, pp. 72-73.

Selon l'habitude généralisée d'arrondir les chiffres (cf. aussi plus haut, p. 74, dans RÉS 3303), p.ex. D.H. Müller dans Südarabische Alterthümer im Kunsthistorischen Hofmuseum, Vienne, 1899, p. 26), les dimensions seraient "17 x 23", chiffres repris par CIH (II, p. 245) et Robin. En fait, mon estampage prouve que c'est 16,6 (à gauche) et 16,7 (à droite) x 22,7 (en haut) et 22,4 (en bas). Aucune autre donnée épigraphique n'a été publiée jusqu'ici. Les lignes de lettres sont gravées entre 3 lignes finement tracées; distance entre les 2 extrêmes: 1,7 cm; distance de la ligne supérieure à la ligne centrale: 0,9 cm; interligne: 0,2 cm; les empattements sont rares ou à peine esquissés et les traits horizontaux et médians de $^{\flat}$, b, h, ḥ, s et ṣ sont remplacés par des courbes concaves ou convexes (cf. déjà RÉS 4674 à la p. 68), et également dans tous les l, excepté celui de la fin de la l.5.

Trois points du texte doivent être mentionnés.

1 - Le moins important est l'illogisme dont Robin fait montre à 3 reprises différentes.

a - Contrairement à sa haine du séparateur de mots dans sa "transcription", Robin reproduit le dernier trait de la 1.7. Ici aussi, inutile de chercher à expliquer ses initiatives insolites; il faudrait s'adresser au djinn de sa girouette. Hélas, son adresse reste toujours inconnue, si tant est qu'elle existe.

b - Robin a retenu le truc du signe d'égalité en fin de ligne pour indiquer la division d'un mot en deux, =. A la fin de la 1.3, = est remplacé par un petit tiret, -, sans que l'on puisse expliquer ce nouveau caprice de sa girouette qui, on doit bien se rendre compte à l'évidence, est le symbole "Robinien" du mouvement perpétuel.

c - Une nouvelle fois, sans en fournir aucune explication, la 1ère lettre de la 1.7, f, est hors de l'alignement des 6 autres lignes.

2 - La 2e critique concerne wqhw (ll. 3-4) que Robin transcrit "wqh- (4) <h> w" à la suite du commentaire de D.H. Müller: "wqhhw", et du CIH qui parle d'haplographie, comme dans CIH 378/5, et que Robin admet avec , toutefois, la réserve d'un "peut-être". La possibilité d'une telle omission de lettre soit par haplographie soit - plus probablement - par simple oubli, n'est pas en cause, car beaucoup de choses sont possibles. Toutefois, lorsque le CIH (p. 246B) affirme que, selon D.H. Müller, wqhw serait "mendosum" = "faulty", la phrase de Müller, savoir "wqhw ist wohl für wqhhw verschrieben,"

n'implique aucune faute volontaire de la part du graveur, car elle n'exclut pas une simple distraction ou inattention. Haplographie ou distraction, encore faut-il que cette possibilité soit justifiée et Robin, pas plus que ses prédécesseurs, n'essaie même pas de le faire. Les 2 phrases de son commentaire (p. 100) ne justifient rien; bien au contraire, elles montrent combien Robin peut déformer un contexte sur la base d'idées préconçues.

3 - La 3e critique concerne la remarque du "pronom suffixe pluriel -hmw (ligne 7) alors qu'on attendrait le duel"; ceci est la 1ère phrase du commentaire de la p.100. Il paraît tout de même bien enfantin et mesquin de fonder le nombre d'un pronom personnel (celui de -hmw de bythmw) sur sa propre interprétation des 2 termes qui le précèdent dans le texte. Il est illogique de vouloir supprimer le pluriel du verbe wqhw des ll. 3-4 en faveur d'un singulier parce qu'on veut changer le 2e pluriel de la 1.7 en un duel. En effet, ce qui est encore plus aberrant, c'est le simplisme de refuser d'admettre que $^{\flat}$hy de $^{\flat}$hyhw (1.6) peut être duel ou pluriel, en plus de singulier. Il ne reste, donc, absolument rien à Robin pour étayer sa suggestion intempestive et malvenue sinon son prurit maladif inné de rechercher l'inhabituel et l'extravagant en fermant les yeux sur ce qui est normal. Rien de plus simple et de plus exact que de respecter la gravure des 2 pluriels de l'auteur qui, après tout - n'en déplaise Robin et consorts - savait ce qu'il faisait et rédigeait un texte dans sa langue maternelle. C'est faire étalage d'une morgue stupide illimitée que de vouloir en remontrer à un auteur sud-arabe non seulement une fois, mais deux fois dans un seul texte très court. C'est là, à mon avis, le summum de l'idiotie et la négation pure et simple de la science objective au profit d'un journalisme neurasthénique désaxé. Ces pluriels s'accordent avec les membres de la maison Ṣarfân de la 1.1, qui est formellement rappelée par bythmw de la 1.7.

La 2e partie de CIH 530 se lit comme suit:

3 -/ḫgn/dt/wqh
4 w/bdt/mt^{c}/dsmwy/^{c}bdh
5 w/$^{\flat}$lwhb/mrḍ/mrḍ/l
6 wfyhw/wwfy/$^{\flat}$hyhw/ww
7 fy/bythmw/

3 -, selon ce qu'ils avaient ordon-
4 né — parce que Dû-Samâwî avait sauvé Son fidè-
5 le $^{\flat}$Ilwahab, de la maladie dont il souffrait — pour
6 son propre salut et le salut de ses frères et le sa-
7 lut de leur [= eux tous] maison.

Les ll. 3-7 développent 3 points concernant le passé de la statue qui perchait au-dessus du bloc de sinter calcaire qui porte aussi le texte.

Ll. 3-4 spécifient que la statue est le résultat de la volonté de la famille Şarfân.
Ll. 4-5 forment une parenthèse explicative précisant que la statue avait été voulue en action de grâce pour la délivrance d'une maladie dont l'auteur fut atteint.
Ll. 5-7 contiennent une pétition concernant le futur: le salut non seulement de l'auteur, mais aussi de ses frères et de leur famille à tous, Şarfân.

h - tmnytn de CIH 546/1.

Bien que paraissant toujours être à l'affût pour dépister un rapprochement, suggérer une comparaison, mettre en valeur un parallèle, tirer des conclusions grandiloquentes et surtout échafauder des esquisses historiques, c'est la 2e fois (cf. aussi plus haut, p. 55) que Robin rate une belle occasion de récidiver à propos de -/tmnytn/wʾbʿl (2)syr/wmfr/hgrn/hrmm de CIH 546/1-2 (pp. 71-72) "- les Excellences et les propriétaires de la plaine et de la campagne de la ville de Haramum, que Robin traduit (p.72;cf. aussi p.52; sans référence l'une à l'autre) par "les Huit et les Anciens du pays et du territoire de la ville de Haramum", traduction grandiloquente qui cadre bien avec son aperçu historique tout aussi pompeux. Ni son commentaire (p. 71), où tmnytn n'est même pas mentionné, ni son texte dans The New Encyclopaedia of Islam (VI [1991], pp. 829A-832A, sur "al-Mathāmina", qui figure dans sa bibliographie, p. 207), ne contiennent de renvoi de l'un à l'autre. L'absence de tout commentaire à la p.72 ne s'explique pas,pas plus d'ailleurs que l'absence de tout renvoi à la p. 55, à moins que ce ne soit le truc bien simple d'ignorer une référence, espérant que le lecteur ne prendra ni le temps ni la peine de chercher la discussion du passage en question dans les "Institutions" (pp. 51-60) et que, conséquemment, il pourrait oublier que le texte porte bien tmnytn et, par conséquent, le groupe ainsi appelé n'appartient pas à hgrn/hrmm comme les wʾbʿl syr/wmfr, ce que suggère tout naturellement la traduction de Robin, "les Huit et les Anciens du pays et du territoire de la ville de Haramum" (p. 72). Cette expression rappelle celle de RÉS 3107bis/1: dtny/ʿšr/wdqʾhʾlt/ʿttr celui des Douze et celui de l'assemblée de ʿAttar. Ce texte est repris à la p. 55 comme suit: "gʾlʿd(l)t bn W(d)d(ʾ)l d-tny ʿs²r w-d-qh(l)t ʿttr, suivi du nota bene: "lecture CR)" (avec la 1ere parenthèse oubliée!). Robin ne spécifie pas sur quoi il fonde sa lecture, et comme c'est son habitude invétérée, il ne spécifie pas davantage la valeur de ses parenthèses. Toutes ces parenthèses sont ... on ne sait que dire puisqu'on n'en connaît pas la valeur: elles doivent être supprimées; et le y de son 2e nom propre est matériellement impossible, et ce nom se lit wynʾl Waynʾil. Ma lecture précitée se base sur mon étude de 1960. De plus, bʿl ne signifie nullement "ancien", comme Robin le prétend dans sa traduction. Il avait d'ailleurs traduit le mot par "possesseur" (une maison)" et "maître" dans Les hautes-terres ..., respectivement, I, p. 183, et II, p. 195. En outre, ces fameux "al-Mathāmina" étaient tout d'abord dans Les hautes-terres ..., I,p. 139, note 212, des "lignages de qayls", mais ils ne sont plus que des "nobles familles" — une expression qui ne signifie rien en sud-arabe puisqu'elle est issue de la conception décrépite moyenâgeuse dont A.F.L. Beeston et son consortium de serfs ont voulu couvrir la société sud-arabe (cf. plus haut,pp.57 et 67) — dans The New Encyclopaedia of Islam ..., l.c., p. 829B. Comme d'habitude, ces 2 métamorphoses ne sont ni expliquées ni même signalées.

L'expression sud-arabe précitée ne confirme pas les hypothèses de Robin sur les "al-Mathāmina" et indique combien les historiens yéménites anciens ont brodé et adapté l'original tmnytn à leur "al-Mathāmina". Robin exclut toute relation avec "the concept of price or value also contained in the root ... since they could also be called al-Thamāniya ('the Eight')", et il croit pouvoir confirmer son interprétation en insistant sur le fait que l'adjectif "*Muthamman or *Muthman " signifie "'repeated eight times'" (cf. The New Ecncyclopaedia ..., l.c., p. 829B). Belle argumentation qui cache tellement de données: pourquoi donc les cacher?;de quoi Robin a-t-il donc peur?

L'adjectif mutamman signfie "précieux" aussi bien qu'octuple;
les adjectifs mutman = mutmin = tamîn signifient "précieux,de grand prix";
le nom mutammin désigne un"estimateur, appréciateur".

Enfin, qu'y a-t-il de surprenant à ce qu'un auteur qui ne cite que 8 noms trouve tout naturel de les appeler "at-tamânîyat" ,mais seulement une seule fois? N'étant pas leur nom caractéristique, cette expression pourrait être une addition tardive, voire même personnelle de l'auteur, destinée à fixer définitivement l'étymologie du mot sur la base des 8 noms retenus. Une fois les lectures corrigées par l'élimination du "Dhū" qui introduit erronément le nom propre sans en faire partie, les traditions arabes ont conservé les noms de 6, 7, 8 et même 9 noms propres (cf. The New Encyclopaedia ..., l.c., p. 830B), parmi lesquels on reconnaît le nom de la ville ṣrwḥ Ṣirwâḥ, et celui de la région mʿfrm Maʿafirum. Il est donc bien présomptueux d'affirmer que tmnytn de CIH 546 signifie nécessairement "huit",- ce que Robin fait -; en tout cas, il serait nécessaire de le prouver - ce que Robin ne fait pas - et non simplement l'affirmer - ce que Robin ne cesse de répéter.-Le mot tmnytn n'étant pas déterminé, comme wʾbʿl, par hgrn/hrmm, désigne donc un organisme interrégional dont l'autorité pouvait s'étendre sur plusieurs centres urbains.

i - Les formes verbales y/thlʾn.

La forme verbale yhlʾn et son parallèle féminin thlʾn interviennent dans une des phrases finales des inscriptions expiatoires au dieu Dû-Samâwî, y compris CIH 568, contrairement à Robin (p. 48). Les 4 contextes sont les suivants:

1 -/wtḫl²n de RÉS 3956/9,

2 -/wtḫl²n/fh(7)dr^ct/w^cnw/fl/ytw(8)bnh/n^cmtm de CIH 568/6-8,

3 -/fhqr^c(9)w^cnw/wyhl[²]n/wlytwbn de CIH 523/8-9, et

4 -/fhqr^ct/w^cnw/w(10)tḫl²n de CIH 532/9-10.

Les flèches renvoient aux péricopes identiques ou parallèles. - La traduction de Robin est la suivante:

1 "Puisse-t-elle être pardonnée!" (p. 104),

2 "Puisse-t-elle être pardonnée! Alors, elle s'est (7) humiliée et affligée. Qu'Il la récompense de faveurs!" (p. 126),

3 "alors il s'est humilié (9) et affligé. Puisse-t-il être pardonné! Qu'Il [le] récompense!" (p. 111) et

4 "alors, elle s'est humiliée et affligée.(10) Puisse-t-elle être pardonnée!" (p. 101).

À remarquer, tout d'abord, la virgule superflue après "Alors" des n°s 2 et 4 contrastant avec son absence au n° 3. — Le schéma précédent montre que l'ordre des verbes de la 4e citation est repris et même élargi dans la 3e, tandis que la 2e reporte en 1ère place le 3e composant du n° 3.

1 - Le commentaire des verbes du n° 4 se limite chez Robin à la répétition des traductions du Sabaic Dictionary ..., suivie d'une remarquable lapalissade sur tḫl²n: "noter le passage de l'accompli à l'inaccompli" (p. 102).

2 - Robin ne justifie pas sa restitution d'un pronom personnel dans le n° 3. Le parallèle du n° 2 qui le mentionne ne vaut pas puisque le pronom fait défaut et l'y restituer revient à substituer sa propre volonté à celle de l'auteur du texte. Cette remarque garde toute sa force bien que la répartition des lettres sur le moule ait été mal réalisée puisque 3 lettres ont été oubliées. Si l'ouvrier avait prévu 16 lettres (au lieu de 15) pour les ll. 6-7 et donné 17 lettres au lieu de 16 (comme à la l.5) à une autre ligne, toutes les lettres eussent pu être présentes (cf. l'axiome de p. 174, début).

3 - Robin donne une autre manifestation de sa girouette capricieuse en traduisant n^cmtm de CIH 528/8 (p. 98) dans un contexte parallèle à celui de CIH 568/8 (cf. plus haut) par "ses faveurs" au lieu de "faveurs", ce qui peut s'expliquer par le même désir de suppléer au texte un adjectif possessif au lieu de zéro, une autre conséquence d'une instabilité imaginative (cf. plus haut, p. 52).

4 - Il est étonnant que les 1ère et 4e formes verbales, ^cnw et hqr^c, puissent être traduites par des formes réfléchies et que les formes y/tḫl²n, qui ne sont jamais introduites par la conjonction l, comme l'est toujours ytwbn, soient données une valeur impréçatoire. La note de Robin insistant sur "le passage de l'accompli et l'inaccompli" ne justifie nullement sa traduction car elle est contredite par la présence de fl avant

ytwbn de CIH 568/7, qui serait superflu puisque, là aussi, il y aurait "le passage de l'accompli à l'inaccompli", savoir w^cnw/wytwbn. La conjonction l- avant ytwbn de CIH 523/9 serait, elle aussi, inutile puisqu'un simple w de coordination joindrait les 2 inaccomplis. De plus, si les péricopes des 3e et 4e citations représentent l'ordre normal des évènements, il n'y a aucune difficulté à y trouver les verbes dans l'ordre chronologique normal. Pour une raison qui reste inconnue et difficile à deviner, cet ordre normal est inversé dans la 2e citation.

5 - La traduction par Robin des termes employés dans ces péricopes est bien caractéristique de l'auteur: la recherche étymologique disparaît au profit de la fantaisie imaginative, comme "être pardonné" pour ḫl². Cf. l'arabe yéménite ḫalâ (o) "nettoyer, purifier, assainir", ḫalá (i) "beauté - acceptable, agréable" (cf. M. Piamenta, Dictionary of Post-classical Yemeni Arabic, Leiden, I [1990], p. 105B). Pour ^cnw, cf. l'arabe ^canâ (o) "devenir humble, résigné, obéissant". Enfin, Robin prend l'abstrait n^cmt pour un pluriel!

Les 4 citations susmentionnées peuvent se traduire de la manière suivante:

1 - Et maintenant elle est purifiée.

2 - Et maintenant elle est purifiée. Et elle (7) fut humiliée et devint humble. Et qu'Il lui [à elle] (8) accorde prospérité!

3 - Et elle fut humiliée (9) et devint humble. Maintenant elle est purifiée et qu'Il soit généreux!

4 - Et elle fut humiliée et devint humble. Et maintenant (10) elle est purifiée.

j - RÉS 2751.

Les 2 textes identiques de RÉS 2751 (pp. 67-68 avec le fac-similé du 1er texte de J. Halévy), mais disposés en sens inverse, l'un dextrogyre et l'autre sénestrogyre, constitutent, selon Robin, "un indice probable d'archaïsme" (pp. 21 = 68). Ce qui, une fois de plus, n'est qu'un jugement tout-à-fait gratuit, le trait le plus marquant de son pamphlet puisqu'on le retrouve presque à jet continu dans ses échafaudages et reconstitutions historiques, sociaux et grammaticaux.

Au dire de J. Halévy (cf. JA, 19 [1872], p. 31), que reproduit Robin (p. 67), "l'une est gravée dans un beau caractère orné". Le tracé qu'il nous en a laissé, appelle les remarques suivantes auxquelles Robin n'a prêté aucune attention.

La graphie utilisée est un double tracé fait de lignes toutes simples, comme dans Kamna 4 (cf. pl. 48). Ces lignes dédoublées devaient être suffisamment rapprochées les unes des autres pour rendre inutile la fermeture de leurs extrémités et impossible l'addition d'oeuillets dans y et t. C'est donc une 1ère erreur de Robin que de boucher les extrémités des 2 d par 4 petits traits horizontaux après avoir, bien gratuitement d'ailleurs, dédoublé le tracé du 2e d. C'est ce double tracé de lignes simples que Halévy dé-

crit comme "un beau caractère orné" pour la simple raison que son tracé ne comporte aucune autre ornamentation. De plus, l'ancienneté des 2 textes vient de la forme du 1er h et du b avec leurs fourches rectilignes en V (cf. plus haut, p. 45), mais la cupule arrondie du 2e h nous éloigne de cette vieille ancienneté. La période de cette inscription peut cependant être précisée davantage: elle est postérieure aux boustrophédons. L'ouvrier n'était pas habitué à graver des textes dextrogyres, car sur les 5 lettres influençables par la direction de la ligne, 3 , c.-à-d. d, l et r, sont bien dextrogyres, mais les 2 autres, ʾ et n, restent sénestrogyres. La direction dextrogyre et sénestrogyre du texte illustre la grande liberté de ces anciens utilisant les 2 tracés dès la plus haute époque, ce qui était déjà bien connu puisque cet usage a permis la création du boustrophédon. De plus, c'était probablement la 1ère fois que l'ouvrier utilisait le double tracé des lettres. S'il n'eut aucune difficulté pour la majorité des lettres et les 2 séparateurs de mots à dédoubler le tracé en utilisant des parallèles, l'appendice central du d posait un problème spécial qu'il solutionna de 2 manières différentes qui ne s'harmonisent pas avec le reste.

Le texte lui-même appelle les remarques suivantes. Le verbe sḫdt est traduit par "a construit de neuf" (p. 68; aussi dans Kamna 7/1, p. 177) = "a fait de neuf" (p.7, ʾInabbaʾ 4/2; cf. plus haut, p. 73). Ce que Robin a voulu exprimer par cette expression reste un mystère, car elle ne signifie rien, pas plus d'ailleurs que "construire de vieux" ou "construire d'ancien" = "faire de vieux" ou "faire d'ancien"; car personne ne construit en utilisant exclusivement des matériaux usagés, et "construire de neuf", par opposition à "construire de vieux" ou "construire d'ancien" est une lapalissade saugrenue. Tout le texte et sa traduction sont déjà donnés, mais sans renvoi de l'un à l'autre, à la p. 21, nouvelle oeuvre d'un caméléon anthropoïde (cf. ici, p. 50):

p. 21: le texte "désigne bien l'ensemble de la construction et non une partie, le portique par exemple"; et

p. 68: "Hdnn: ce nom a toute chance d'être celui du temple, mais il n'est pas totalement exclu que ce soit celui du seul portique".

Il ne faut tout de même pas oublier que Halévy parle de la "porte du sanctuaire" et nullement de son "portique". Le fait que le texte est reproduit de chaque côté de la porte indique bien qu'il s'agit des 2 bastions flanquant la porte Hadnân, et certainement pas du temple ou du portique qui n'a pas de porte, puisque le portique est par définition une "galerie ouverte au niveau du sol et dont le plafond est soutenu par des supports isolés ou des arcades"

(cf. O. Aurenche, Dictionnaire illustré multilingue de l'architecture du Proche Orient Ancien, Lyon, 1977, p. 145A). Enfin, contrairement à l'affirmation de Robin (p. 68), l'auteur du texte n'est pas, ne peut pas être un "roi" (contre p. 25). Il n'entre pas dans le protocole royal ou dans la psychologie royale d'avoir l'humilité d'omettre son titre.

k - al-Ḫarâšif 3 (pp. 200-201 et pl. 59b).

Le petit texte d'al-Ḫarâšif 3 est une autre splendide illustration de la méthode alambiquée de Robin. C'est la dernière inscription du pamphlet; elle est devenue l'avant-dernière de ce compte rendu qui, sans l'avoir cherché, passe en revue les 3 textes de l'endroit, c.-à-d. le n° 1 aux pp. 58-59, le n° 2 aux pp. 57-58, et le n° 3 ci-après.

1 - Le séparateur de mots est éliminé régulièrement par Robin; c'est réellement sa bête noire, on le sait. Cependant, il s'échappe parfois de la poubelle (cf. plus haut, p. 52). Après mûres réflexions, il pourrait tout de même se faire que ces illogismes se produisent lorsque Robin n'est pas encore tout à fait bien éveillé. Ici Robin coupe la poire en 2 - nouvel exemple d'une instabilité congénitale déjà mentionnée plus haut (p. 80). Sans justifier sa demi-mesure, il renseigne le séparateur de mots au début des ll. 1 et 3, et il ignore les 8 autres. Le cas de CIH 530 est donc bel et bien dépassé en sorte que, tout bien raisonné, il serait peut-être indiqué de voir en Robin le témoin de l'anthropoïde caméléon (cf. plus haut, p. 82).

2 - Ici encore, la photo de Robin ne permet pas de voir clairement toutes les lettres, surtout aux extrémités; p.ex., la lettre initiale de la l.1 est lue "ṣ" par Robin. Toutefois, le trait oblique droit d'un ṣ ne se voit pas sur la photo, et ce qui pourrait (?) être celui de gauche peut n'être que le rebord d'une veine ou d'une aspérité. Donc, jusqu'à preuve d'une photo digne de ce nom, je lis ybḥn; cf. bḥn, nom de personne minéenne dans JsaL 90, et d'une famille sabéenne dans Kortler 7 c/2.

3 - Le commentaire du nom "Ṣbḥn" que Robin lit au début de la l.1 illustre une fois de plus une fausse accusation (la 2e!; cf. déjà plus haut, pp. 41-42) et aussi la superficialité de sa méthode de travail (cf. plus haut; pp. 42-43). Robin s'en réfère à "Ṣbḥn" de CIH 287/14 (= NāmĪ NNSQ 58 qui lit Ṣbḥm)" (p. 201). Commentaire ahurissant, s'il en est un! En effet, CIH 287/14 a ṣbḥm dans CIH, II, commentaire de p. 298B, et translittération en lettres hébraïques à la p. 299, et H.Y. Nâmî (cf. Naẓr nuqûš ṣâmîyat ..., Le Caire, 1943, p. 73) a bien ṣbḥm dans sa translittération en lettres sud-arabes et arabes (cf. déjà plus haut). En plus, la lecture ṣbḥm est déjà attestée par M. Hartmann, Der arabische Frage, Berlin, 1909, p.259: "Ṣabbaḥum". Où donc Robin a-t-il pu pêcher sa lecture "ṢbḥnṢbḥn"? La chose est à peine croyable: il l'a copiée dans CIH, II, p. 296, l.8, la transcription en lettres sud-arabes qui, comme l'indiquent les 2 références du CIH mentionnées plus haut, ne peut être qu'une coquille. Voilà donc Robin accusant H.Y. Nâmî d'une erreur de copie sur la base d'une coquille du CIH qu'il aurait pu et dû dépister simplement en tournant une seule page du CIH et en lisant le commentaire et la translittération hébraïque. Il convient également de faire remarquer que les références de Robin à son "ṢbḥnṢbḥn" restent incomplètes.

4 - Robin continue sa folle randonnée dans al-Ḥarāšif 3 en appliquant aveuglément la théorie de bn fils de à la carte d'identité de l'auteur et, de ce fait, le 2e nom après bn ne peut être que celui d'un associé de l'auteur du texte. Toutefois, ce qui complique considérablement la situation, c'est que, selon Robin, ce 2e nom "pourrait être le nom antique d'al-Ḥarāshif" (p. 201), sans présenter d'ailleurs la moindre considération susceptible de prouver la plausibilité de son idée inconsidérée. Il est cependant aisé de suivre le processus de son raisonnement. D'une part, ce 2e nom, tḥty, est donné à la l.3 comme celui des subordonnés de l'auteur de l'inscription qui en était le kbr. D'autre part, l'interprétation de bn (l.1) par Robin excluait automatiquement la possibilité que les 2 noms introduits par bn puissent être des noms de groupes ethniques. Il ne lui restait donc plus qu'à voir dans l'inconnu tḥty "le nom antique d'al-Ḥarāshif" (p. 201), conséquemment, le sujet du verbe devait être, selon l'auteur, un individu et une ville! Il est regrettable que Robin n'ait pas donné un seul parallèle étayant son intuition bizarre! En fait, le mot bn introduit les noms de deux familles ou petits groupes auxquels le sujet appartenait (cf. plus bas, le commentaire).

5 - La dualité de sujets, soutenue par Robin, est contredite par le singulier du verbe rṯf. Il est bien connu, cependant, que Robin, qui jongle avec les textes avec la même désinvolture et le même dévergondage qui caractérisent la manipulation des lettres par J. Pirenne, en est arrivé à se persuader que le verbe singulier qtl soit "duel ou pluriel", une pillule combien attrayante avalée sans coup férir et même avec délices par les suppôts du "laudemus invicem" d'A.F.L. Beeston et les béni-oui-oui modernes de C. Robin; cf. plus loin, pp. 178-184, 208-209 et 225-226.

6 - Le nouveau verbe rṯf est employé isolément sur une pierre remployée dans un puits, et aucune ancienne bâtisse n'existe dans les environs. Dès lors, on s'étonne que, retenant le parallèle le plus obvie, rṣf, Robin traduise rṯf par "ont élevé (ce portique) (?)". Sur quoi se base cette traduction du verbe? On l'ignore. D'où vient "ce portique" sinon de la réminiscence persistante de RÉS 2751? (cf. plus haut, p. 82). Pour rṣf, cf. raṣafa en yéménite, 1ère forme "mettre en ordre à" et 2e forme "rassembler", et l'arabe "paver", qui assurément implique les 2 significations yéménites.

7 - Les 2 personnes aux noms de ʿmyṯʿ et ʿmšfq introduites par la formule de datation bywm au temps de (cf. plus haut, p. 59), sont des personnages officiels interprétés tout naturellement comme les chefs respectifs des 2 groupes ethniques mentionnés à la l.1.

8 - Contrairement au bourrage de crâne de son pamphlet le présentant comme le gourou qui résoud et aplanit toute difficulté, Robin donne un nouvel exemple de sa girouette vacillante (cf. en dernier lieu, bʿl aux pp. 78-79) en prônant simultanément 2 interprétations de kbr, c.-à-d. soit la traduction du terme en "chef", soit la simple vocalisation du mot en "kabīr" ... et pour que les 2 systèmes puissent cohabiter en parfaite symbiose dans son pamphlet, il attribue 4 péricopes à chacun d'eux, comme l'indique le tableau de la p. 52. Toutefois, ici encore et comme toujours, il est inutile de chercher quel pourrait être le critère justificatif d'un choix plutôt que de l'autre dans chaque cas en particulier, car on ne peut perdre de vue la vérité déjà mentionnée plus haut, savoir que Robin ne doit d'explication à personne (cf. déjà plus haut, pp. 46, 47, 54 et 56).

Le texte d'al-Ḥarāšif 3 se lit comme suit:

1 ⌜⌒ybḥn/bn/ṣdqsmʿ/wtḥ⌝ṭ⌝
2 y/rṭf/bywm/ʿmyṭʿ
3 ⌟w ʿmšfq/wkbr/tḥty

1 Yabawḥān, issu de(s 2 groupes) Ṣaduqsāmiʿ et Taḥt-
2 ay, a pavé [ceci] au temps de ʿAmmyaṭaʿ
3 et de ʿAmmšafaq, et [au temps où] il était le leader de Taḥtay.

Comme c'est souvent le cas en qatabnite, bn introduit ici les noms désignant la famille, le groupe ou le clan auquel l'individu appartient par naissance, par opposition à d qui indique une appartenance non basée sur le sang.

Une dernière note sur kbr (cf. plus haut) exemplifie le bluff de la méthode de Robin et s'ajoute aux autres remarques déjà formulées; elle est donc bien à sa place ici dans ce compte rendu.

Dans sa thèse dactylographiée Le pays de Hamdān ..., le mot kbr mentionné dans "Index ... des termes reinterpretes" (en majuscules; p. 574) est renvoyé à "MAFY/Ḥamir 3,2" (p. 593) = RoPH 69/2, qui mentionne seulement ...]ḥ/kbrhm/- (p. 218). Robin restitue ce fragment, bien gratuitement d'ailleurs, en ft]ḥ/kbrhm/-" ... la sent]ence de leur kabir", et le commentaire fait donner tous les cuivres du Grand Orchestre de Paris pour proclamer à la face du monde que "ce texte laisse entrevoir que l'interdiction est édictée par le kabir ...Il nous apprend donc l'existence de kabir à Ḥamir" (p. 218).

J. Pirenne n'aurait pas manqué de contresigner une grandiloquence aussi vaine. Le lecteur note tout naturellement que le mot kbr est vocalisé "kabir" avec 2 voyelles brèves; mais la publication officielle Les hautes-terres ... transcrit toujours "kabīr" avec un long "ī", comme l'indique l'"Index des mots sudarabiques definis ou reexamines" (en majuscules, I, p. 183), où le lecteur trouve ce qui suit:

"'kabīrs': 87. Voir kbr.
kbr, 'kabīr': 76. Voir kabīrs" (p. 184, ll. 1-2).

Et tout ce que la p. 87 contient est l'affirmation d'après N. Rhodokanakis de "la place des kabīrs à la tête des tribus" avec renvoi à la note 234 (p. 140) qui, elle, s'en réfère à la note 18 (p. 123), c.-à-d. 'Voir la vie de al-Hamdānī...'! Soit dit

en passant, le lecteur note encore une autre prouesse de la girouette de Robin, le symbole du mouvement perpétuel: comparer le "de al-Hamdānī" de la citation précédente avec les mentions de "d'al-Kāfir" (p.ex., p. 129) et "d'al-Ḥarāshif" (p.ex., p. 197).

Et tout ce que la p. 76 mentionne est "les fonctions de kbr (MAFY-Ḥamir 3,2)". C'est tout, car ce texte n'est pas repris dans ce volume. On constate aussi que la girouette de Robin a été hyperactive (une fois de plus!) car "MAFY/Ḥamir" de la thèse est devenu "MAFY-Ḥamir", ce qui est certainement une amélioration de toute 1ère importance et combien nécessaire!

Voilà donc où aboutit le charabia de 2 séries de références de Robin, qui ne sont rien d'autre que 2 pincées de poivre jetées à la face du lecteur.

Ce qui est plus grave encore, c'est que l'hypothèse de la théorie basée sur une restitution gratuite et improuvable de texte est maintenant présentée comme une vérité indiscutable. Voilà la méthode de travail de Robin! Au fond, comme ce compte rendu l'explicite déjà plus haut à plusieurs reprises (cf. p. 84) et continuera à le répéter, Robin brasse les textes avec la désinvolture et l'impertinence qui caractérisent les études paléographiques de J. Pirenne.

1 - Fa 127 (pp. 122-123).

Le dernier texte sur lequel mon regard s'est attardé pour une raison à ce point furtive qu'elle m'échappe pour l'instant - à moins que ce ne soit l'aspect des caractères sud-arabes trapus, aplatis et ajourés contrastant tellement avec ceux étriqués utilisés par Robin et qui rappellent les échasses des animaux gratte-ciel, les girafes et les sveltes cigognes, hérons, etc. - est un texte copié et photographié par A. Fakhry en 1947 et publié par G. Ryckmans dans Epigraphical Texts, Cairo, 1952, pp. 78-79, après l'étude de la photo originale. Une lecture superficielle de la traduction et du commentaire de Robin montre que son traitement du texte peut être considéré comme un exemple typique du travail "Robinien".

L. 1. - Le nom de l'auteur est "mnhm" sur le fac-similé; Robin le corrige en "mn<b>h^{m}" sans aucune justification bien que G. Ryckmans le donne comme "mnbhm" dans sa translittération et son commentaire qui affirme clairement "mnbhm; la lecture apparaît clairement sur la photographie" (p. 78), et le rende comme "Munabbihum" (cf. l.c.). Voilà donc Robin n'informant même pas son lecteur du témoignage formel de G. Ryckmans qui écrit que "La copie de Fakhry a été collationnée sur une photographie lisible, mais trop peu distincte pour qu'il soit possible de la reproduire" (p. 78).

Il avait mille fois raison, M. Rodinson, de dire qu'"il y a des obscurantistes partout" (cf. plus haut, p. 37): il en a formé un lui-même, c.-à-d. non seulement Robin cache la vérité à son lecteur, mais, ce qui est bien pire, il agit à l'encontre! En outre, c'est

toujours la même pagaïe dans les petits gadgets typographiques dont Robin affuble ses translittérations et ses commentaires: il n'y a aucune différence entre < > et [] vu l'identité de ce qu'ils représentent; p.ex. "Mn<b>h^{m}" et "bn[y]" de la l.1; dans les 2 cas, les lettres manquent sur le fac-similé. Est-ce que, par hasard, la différence ne serait pas que le 1er mot se trouve en tête de la ligne, et le 2e tout à la fin? De plus, Robin ne fait pas remarquer que

1) les 2 -hmw, des pluriels, se rapportent seulement à un duel, mnbhm et šllm;

2) šllm est le nom de l'épouse du 1er; et

3) bnw et ʾḫnkn sont 2 pluriels.

Pour mnbhm, cf. l'arabe manbūh "connu et célèbre".

Ll. 2-3. - La restitution de "[l]wf[yh]mw se retrouve déjà dans le texte de G. Ryckmans; Robin n'a fait que la copier sans rien dire. wfy est traduit erronément 2 fois en "sauvegarde", une idée que Sabaic Dictionary ..., p. 158, conserve comme un sens du verbe à la 10e forme. Le terme "sauvegarde" ne se rapporte qu'à un moyen d'obtenir le "salut," mais n'exprime nullement qu'elle fut ou serait bénéficiaire. Donc, jusqu'ici, la transcription de Robin est en recul très net sur celle de G. Ryckmans; elle ne mérite aucune considération. - qnyw et yqnynn: le verbe qny est traduit 2 fois erronément par "acquérir;" Sabaic Dictionary ..., p. 106 + p.1 des "Addenda et corrigenda" donne "acquérir, posséder", comme si ces verbes étaient synonymes; "acquérir" est ʿsy.

L. 3. - L'interprétation de Robin de "mnbhm" comme commençant une phrase, "Quant à Munabbih^{um}", n'est pas son invention bien qu'il n'en souffle mot; il s'attribue doucement et sans rien dire une idée d'A.F.L. Beeston dans son compte rendu de G. Ryckmans paru dans BSOAS, 16 (1954), p. 396. En outre, comme s'il travaillait encore à moitié endormi, le pronom pluriel -hmw annexés à ʾḫy et à bny (ll. 1 et 3), se rapportant à deux personnes, mnbhm et šllm, est traduit respectivement par "leurs" (l. 1) et "ses" (l.3) par Robin (p. 123), et w est éliminé!

En outre, ici encore, un nouvel exemple de la pagaïe des petits trucs typographiques de Robin est son "M*n*bh^{m}" de la l.3. Les 2 astérisques ne signifient rien et ne se justifient pas parce que m et n sont certains sur la copie de G. Ryckmans (cf. son témoignage formel reproduit plus haut, p. 86).

La péricope wmnbhm/wʾḫhmw/wbnyhmw dépend de wfy de la même ligne, une répétition qui désigne bien que c'étaient ces personnes que l'auteur désirait recommander tout spécialement à la bienveillance du dieu Dū-Samāwī.

Finalement, on peut se demander si Robin ne nous montre pas le chemin et ne nous suggère pas indirectement, mais sûrement, que ses écrits et concoctions soient, elles aussi, jetées à la poubelle, comme il fait de ceux d'A.F.L. Beeston, G. Ryckmans, et autres. Ils le méritent bien.

L. 4. - Robin fait la remarque suivante: "wld: si la copie de l'inscriptions est exacte, ce mot est apposé à ʿAthtar" (p. 123). Revoici, Robin récusant une nouvelle fois le témoignage de G. Ryckmans ... mais sans le dire ouvertement! Il y tient vraiment à maintenir son placard bien rempli de détritus! Car, s'il s'imagine que le wldhmw de la copie n'est pas exact, de quel droit relègue-t-il dans ce même dépotoir, sans le signaler bien entendu, la suggestion d'A.F.L. Beeston selon laquelle "wgdhmw 'their ancestor'" se lirait au lieu de wldhmw, ce qui éliminerait du même coup l'opposition suggérée par lui-même? Mais Robin endorse de fait l'idée de Beeston vu que ce wld "pourrait être un participe présent: voir l'arabe wālid". Malheureusement, la question n'est nullement l'existence de l'arabe wâlid, qui n'a rien à voir ici et n'est qu'un cheveu dans la bouillabaisse provençale de Robin puisqu'il s'agit d'un texte sud-arabe où - ici encore, Robin redevient muet comme une carpe - le substantif wld attesté de très nombreuses fois ne signifie jamais "père, procréateur". Que de verbiages complètement inutiles à cause d'un fanatique pan-arabiste classique, intrus en sud-arabe ... pour un minuscule w, comme si la solution la plus simple n'était pas de supposer l'omission de la copule w par asyndète ET de retrouver aux ll. 3-4 un autre cas de cette construction asyndétique, c.-à-d. une longue énumération coupée en 2 pour mettre en évidence dans la 2e section ce qui paraît être particulièrement important pour l'auteur:

l. 3: -/wwfy [[non seulement]] (1) ʾwldhmw/,
w + dqnyw/,
w +[d]yqnynn/,
[[mais aussi]] (2) w + mnbhm/,
w + ʾhyhmw/,
w + bnyhmw.

ll. 3-4: -/wrtdw [[non seulement]] (1) mnṣ[bt]/wmḥrm/ʾlhmw/dsmwy/ʿttr/ṯsrqn/
[[mais aussi]] (2) + wldhmw/ḥmyt/,
w + ʾhlt/hrmm.

La construction asyndétique est connue en minéen: ʿsy/bny de RÉS 3112/1, en sabéen; p.ex. RÉS 4905/2: bny/kl had built [and] erected dans mes Miscellanées ..., XIV (1985), pp. 281-282, où les erreurs combinées de C. Robin et J. Ryckmans sont dénoncées et réfutées, et surtout en qatabanite; p.ex. ʿsy/ẓrb de Ja 331/1, et ʿsy/ẓrb/bny de Ja 343/2, à comparer avec ʿsy/wẓrb/wbny de Ja 118/1. Cf. below, p. 181, the asyndeton of 2 complements

Ici aussi Robin brouille les cartes et se noie dans une tasse d'eau (cf. déjà, p. 49); il écrit: "après le verbe rtd, on attendrait plutôt la mention de divinités" (p. 123). Il prouve une fois de plus qu'il ne retient que ce qui lui plaît, c.-à-d. il ne fait état

que d'un seul type de péricope, d'ailleurs fréquent, p.ex., wrtdw/h(20)qnyn/ʿttr/ṯsrqn/wʾlmqh/bʿl/ʾwm (Ja 559/19-20). Mais, il y en a bien d'autres; p.ex.,
wrtdw/bnw/ʿblm/ʾlmqh/hqnythmw/bn/hkrnh(8)<w>ḏrynh (Ja 558/7-8), and
wrtdw/ʾlmqh/thwn/bʿl/(12)ʾwm/hqnythmw/bn/mʾḫrm/wmśwrm/wdśśm (Ja 703/11-12).

C'est sur la base d'une interprétation erronée de la syntaxe du texte que Robin fonde sa nouvelle explication de ʾhlht comme le pluriel de ʾl. En fait, Robin invente une forme sud-arabe de ʾl qui, comme son wld de plus haut, bien que mentionné de très nombreuses fois, ne s'était jamais rencontrée et n'est nullement supportée par le texte. L'argumentation de Robin est répétée presque textuellement à la p. 73 (cf. aussi pp. 14, 33 et 34) dans CIH 546/5 où le contexte s'y oppose formellement parce que bsr/hrmm (l.4) dans la plaine de Haramum est opposé à trzʾ/ʾhlhtn (l.5) les dépenses des familles. On ne voit vraiment pas ce que les "dieux" viendraient faire dans un contexte de ce genre au lieu de "familles".

La plus ridicule suggestion de Robin est de voir une nouvelle divinité dans ḥmyt "Protecteur", exactement comme l'arabe ḥamîyat. Mais, ce ḥamîyat, étant le féminin de ḥâmin, ne pourrait désigner qu'une déesse. En fait, c'est tout simplement le nom de l'enfant préféré de l'auteur de l'inscription. ḥmyt est bien connu comme nom d'homme en ḥaḍramoutique Ja 2878 b/4, en minéen RÉS 2810, en qatabanite, p.ex. RÉS 3902, n° 104, et en sabéen CIH 708/1.

Il est bien frustrant de ne pouvoir entrer en relation avec le djinn de Robin pour s'enquérir de sa santé mentale. Cette remarque, pour cruelle qu'elle puisse paraître, n'en est pas moins justifiée du fait que la flèche décisive, c'est lui-même, Christian Robin - à moins que ce ne soit son djinn pour lui jouer un malain tour - qui la posa sur l'arc du lecteur. Qu'il le veuille ou non, le lecteur ne peut que rester bouche bée et abasourdi par la nature répulsive du crible dont Robin se sert, une fois de plus, pour retenir ce qui lui plaît et mettre au rancard ce qui lui disconvient.

D'une part, il s'appesantit aux pp. 14, 33, 34, 73 et 123 pour mieux enfoncer son clou et multiplier ses affiches publicitaires célébrant sa découverte de ʾhlht = dieux dans 2 textes, CIH 546 et Fakhry 127. Encore convient-il de spécifier que, dans ce cas, Robin fait preuve d'une habileté surprenante de changer son fusil d'épaule qui prouve à l'évidence son statut de l'anthropoïde caméléon (cf. déjà plus haut, p. 52).

P. 73, dans CIH 546/5: "il est plus vraisemblable que ce soit le pluriel de ʾlh, comme dans [] Fakhry 127/4".

P. 123, dans Fakhry 127/4: "ne serait-ce pas un pluriel de ʾl, avec le premier h notant une voyelle longue (arabe alifa)? Voir déjà [] CIH 546/5 et commentaire".

On arrive toujours au but, que le point de départ soit ʾl ou ʾlh! La différence de nuance entre les 2 présentations précédentes se retrouve ailleurs avec des écarts plus grands

encore. P. 14: aucun doute n'existe; c'est la certitude parfaite ou la "possessio pacata":"Fa 127 ... 'aux dieux de Haram' (ʾhlht Hrm^{m})". Note également l'escamotage en douceur de la mimation de hrmm dans la traduction!

P. 33: la certitude s'est volatilisée; le doute apparaît:

"CIH 546 ... ʾhlht (probablement le pluriel de ʾlh...; l.5)."

P. 34: la certitude reste toujours absente; le doute est maintenu:

"Fa 127: ʾhlht (l. 4),comme dans...CIH 546/5",contrairement à la p. 14!!!

En un mot, Robin peut jouer avec l'Académie comme il l'entend et lui faire croire que la girouette ou jouer à l'anthropoïde caméléon reste le meilleur expédient pour éviter une difficulté. Pour sa part, le lecteur ne se prive pas de faire comprendre à Robin et à l'Académie que le pauvre d'esprit n'est pas celui qu'ils pensent.

D'autre part, il est, à mon sens, absolument incroyable qu'un auteur puisse publier à la même page que le 1er exposé de sa découverte (?), p. 73, un texte mentionnant la formule normale ʾlʾlt/hrmm les dieux de Haramum de RÉS 2740/11

APRÈS avoir publié la même expression 2 fois auparavant, à 2 et 3 pages seulement de différence, pp. 70 et 71, ʾlʾlt/hrm dans CIH 514/5-6 (p. 70) et 513/5-6 (p. 71),

ET DANS LES PAGES SUIVANTES, il publie cette expression QUATRE fois, y compris dans UNE restitution de texte de son propre cru, savoir

ʾl(8)ʾlt/hrmm de CIH 510/7-8 (p. 86);
[ʾlʾlt](9)hrm[m] de CIH 511/8-9 (p. 87);
[ʾlʾ](9)lt/hr[mm] de CIH 515/8-9 (p. 88) et
[ʾlʾ](7)lt/hrm de RÉS 2749/6-7 (p. 89).

Voilà donc déjà s i x textes attestant comme une partie bien vivante et parfaitement intégrée dans le vocabulaire journalier de ces gens l'expression ʾlʾlt/hrm(m) que Robin, lui-même, reprend dans son pamphlet, dont il ne souffle mot dans sa discussion de ʾhlht/hrmm, et qu'il rejette effrontément pour jouir en toute transquillité et dans le plus grand calme de sa découverte béate, tout en foulant aux pieds l'objectivité due au lecteur. Mis en face de déviations méthodologiques aussi brutales, le lecteur n'a pas de peine à se rendre compte que le grand principe de liberté a été dépravé et abâtardi en un libertinage cynique marqué au coin de la spécificité de chaque égoïsme en particulier, et l'individu se croit tout permis sous le regard approbateur de l'Académie et du CNRS.

Fa 127 se lit comme suit:

1 mnbhm/bn/slmn/wšllm/wʾḫyhmw/wbnyhmw/bnw/ʿbysm/ʾḫnkn/bn[y]
2 w/whwṯrn/whšqrn/mnṣbt/wmḫrm/ʾlhhmw/dsmwy/bʿl/mwqṯn/[l]wf[yh]
3 mw/wwfy/ʾwldhmw/wdqnyw/wyqnynn/wmnbhm/wʾḫyhmw/wbnyhmw/wrṯdw/mnṣ[bt]
4 wmḫrm/ʾlhhmw/dsmwy/ʿttr/šrqn/wldhmw/ḫmyt/wʾhlht/hrmm

1 Manbûhum, fils de Salmân, et [son épouse] Šallum, et leurs frères et leurs
fils, les descendants de ʿUbaysum, des Ḥanakites, [ont] cons-
2 truit et posé les fondations et couvert les piliers et le temple de leur dieu
Dû-Samâwî, le maître de Mawqaṭân, [pour le]ur sa-
3 [lut] et le salut [[non seulement]] de leurs enfants et de ce qu'ils ont possédé
et posséderont, mais [[aussi]] de Manbûhum et de leurs frères et de leurs
fils. Et ils ont mis [[non seulement]] [les] pili[ers]
4 et le temple de leur dieu Dû-Samâwî sous la protection de ʿAṯtar Šarqân , mais
[[aussi]] leur enfant Ḥamîyat et les familles de Haramum.

10 - Appendice: l'anépigraphe al-Kâfir 33.

Le grand insoupçonné et inattendu de la collection intitulée les "Inscriptions" est sans conteste al-Kâfir 33 (p. 146 et pl. 39b), le seul anépigraphe du pamphlet, dont la présentation de moins de 2 lignes de texte, 1 ligne et demi pour être exact, est un modèle du laconisme d'un je-m'en-fichiste de tous les jours.

La "description" se limite à dire qu'il s'agit d'un "graffite médiocre représentant deux mains ouvertes et un serpent ondulant". C'est tout: aucune référence à une quelconque étude signalant pareils pétroglyphes,p.ex., A. Grohmann, Göttersymbole ..., pp. 44-46 (main) et 71-73 (serpent); aucune dimension, rien! — Les mains montrent la paume avec les doigts bien écartés, pointant vers le bas (main supérieure) ou le haut (main inférieure). — L'interprétation de la ligne tracée sous et à gauche de la main inférieure comme représentant un "serpent ondulant" est inexacte: la ligne se compose d'au moins 7 plis anguleux, dont 2 ont moins de 90°, tous angles physiquement impossibles pour un serpent.— La mauvaise qualité de la photo ne permet pas de voir s'il y a d'autres dessins de main; p.ex., sous et à droite de la ligne anguleuse. Enfin, Robin cite bonnement ce pétroglyphe parmi les textes dans sa "Concordance des inscriptions" à la p. 215, et en 1ère ligne encore! Belle apothéose d'un travail imaginaire! Robin peut enfin se gober à fond : l'Académie endosse tous les frais(cf.plus haut,p.50).

11 - Lacunes et déficience majeures.

La 1ère lacune regrettable qui empêche l'utilisation normale du pamphlet de Robin est la même que celle qui, depuis plus d'un siècle, entrave journellement la consultation régulière du1^{er} Corpus :à savoir l'absence de tout index de mots et noms propres des 117 textes répartis sur 107 (des 221) pages de texte, et dont il est impossible de prévoir le dialecte, minéen ou sabéen, à qui Robin attribue tel ou tel texte. C'est également faire preuve de snobisme et de folie des grandeurs que de rebaptiser minéen en "madhabien" - une appellation niaisement reprise par ses suppôts! - et sabéen en "pseudo-sabéen" (pp. 29 et 34). Cette dernière expression exprime bien l'imbroglio de Robin, car c'est ce qu'il appela jadis le "sabéen" dans Semitica, 28 (1978), p.107;

cf. mes Miscellanées..., X (1979), pp. 36-37. Si, donc, il comprend bien la vraie signification de "pseudo" et puisque tous ces textes sont rédigés en sabéen, il avoue par-là même ne pas savoir à quel dialecte ils appartiennent, auquel cas il aurait dû les ranger en "Robinien" du nom de l'auteur (cf. ici, p. 98). Robin désire sans doute rivaliser avec A.F.L. Beeston et son "Ṣayhadic" que, lui-même, il s'est gardé d'adopter. Robin désire aussi sans doute continuer à être à la page, car ce genre d'innovation est courant en nord-arabe, où les féeriques E.A. Knauf, W.W. Müller, M.C.A. Macdonald, etc., s'en donnent à coeur joie, comme il l'a fait lui-même avec son "qaḥṭânite" (cf. mes Miscellanées..., l.c.). Le reproche précédent concernant un index lexicographique et onomastique garde toute sa valeur nonobstant les listes onomastiques de Haram (pp. 35-39, 53 et 56) et de Kamna (pp. 163-164), qui sont à ce point déchiquetées que le lecteur doit perdre un temps précieux à chercher un seul nom propre. Car il n'y trouvera que des noms propres, la lexicographie n'existant pas pour Robin. Et il s'est même abstenu de dresser les listes onomastiques des textes d'al-Kâfir (pp. 131-146) et d'al-Ḥarâšif (pp. 199-201).

Une déficience sérieuse est la maltraitance des textes de ces 2 dernières collections d'al-Kâfir et d'al-Ḥarâšif, qui sont des plus malmenées du point de vue épigraphique; elle est au diapason de son étude des graffites d'al-Ḥazâ'in (cf. mon compte rendu dans mes Miscellanées..., X [1979], pp. 34-70 et pl. 6-7; cf. plus haut, p. 58). Il faudra bien tôt ou tard que l'Académie envoie sur place un épigraphiste digne de ce nom pour faire le travail muséographique et épigraphique que Robin a bâclé avec une désinvolture parfaite (cf. déjà plus haut, pp. 46-47), c.-à-d. "à la Robin".

La 2e lacune est le manque absolu de liste d'abréviations. Le "N.B." de la p. 209 renvoie le lecteur à 2 ouvrages "pour résoudre les sigles d'inscriptions". Le lecteur pourra toujours chercher le "M" (p.ex. p. 67) et aussi le rébarbatif "MAFRAY-ash-Shaqab 1 et 2" (p. 177), un sigle qui témoigne de la folie, non des grandeurs, mais des longueurs de Robin et qui n'a rien à envier à ceux, tout aussi repoussants, du CIASA de Pirenne, qui lui ont d'ailleurs servi de modèle. C'est de la paresse intellectuelle en même temps que de la malignité envers le lecteur que de le forcer à fouiller ailleurs pour trouver ce que quelques lignes supplémentaires lui eussent fourni aisément. Mais Robin s'est refusé à lui rendre ce service nononbstant le gaspillage effréné de papier dans le pamphlet. Le lecteur n'a qu'à parcourir, p.ex., les 8 pages 209-216 pour s'en rendre compte: l'espace de 3 lignes données à une concordance aujourd'hui inutile: "Grifini 1" (p. 212); les 2/3 de p. 209, p. 210, et la 1/2 de p. 216 sont en blanc; 2 colonnes au lieu de 4 aux pp. 211-216; plus des 4/5 de p. 214 perdus pour la cote "M" qui est injustifiable, inutile et ne figure même pas dans le pamphlet (p. 206) et pas davantage

dans les 2 ouvrages renseignés (cf. plus, p. 92); son utilisation est une basse flatterie de la nouvelle cella académique aux petits frères du sud (cf. déjà ici, p. 73). Du lecteur, Robin s'en fout. Pourquoi répéter 2 fois "Hal 160", "Hal 161" (pp. 211 et 212) et "Hal 164" et "161" (pp. 214 et 215)? Pourquoi utiliser le sigle "RAS" une seule fois (p. 215) et laisser de côté les sigles des musées de Hambourg, Vienne et Londres? N'est-ce pas ridicule d'utiliser le sigle "RES" (pp. 215-216: 41 fois) et ne mentionner que Répertoire : Répertoire d'Épigraphie Sémitique" dans la bibliographie (p. 207)? Cette remarque vaut également pour le sigle "CIH" (pp. 211-212: 31 fois) et "Corpus: Corpus ..." (p. 204). Par contre, le sigle en accordéon "MAFRAY ..." (pp. 214-215: 5 fois seulement) est bel et bien explicité à la p. 206, ce qui relève de l'égoïsme chauvin franchouillard.

Il est bien certain que peu de lignes eussent suffi pour imprimer ces 2 listes manquantes en lieu et place des innombrables développements imaginatifs, concoctions et contradictions de Robin. En effet, si le pamphlet avait été soumis à un réviseur compétent en vue d'une épuration rationnelle et systématique de toute "Robinerie", le pamphlet aurait considérablement fondu et serait devenu beaucoup moins coûteux aussi bien pour l'Académie que pour le lecteur. Malheureusement, toute idée de contrôle et de révision était rendue impensable par la pompeuse dithyrambe de la "Préface". Laissé à lui-même et rangaillardi par la certitude d'un placet aussi parfait qu'aveugle, Robin put enfin réaliser la seule chose qui lui tient à coeur et qu'il allait pouvoir finalement accomplir aux frais de la princesse, savoir satisfaire pleinement toutes les exigences de la folle du logis (cf. déjà ici, p. 91). Et le lecteur? Qu'il se débrouille!

12 - La composition du fascicule B.

Le fascicule B intitulé "LES PLANCHES" (couverture et p.V) est pour ainsi dire jeté à la tête du lecteur ignorant de ce qu'il va y trouver. Comme Robin a écrit tellement de choses saugrenues, surprenantes et hors de propos, le lecteur n'a aucune idée de ce qu'il va trouver dans ces 60 pages.

La toute 1ère critique est qu'il eût été avantageux de donner une numérotation suivie à ces illustrations au lieu de les affubler d'une lettre, a et b, c, d suivant que la planche compte 2, 3 ou 4 photos, p.ex., pl. 2, 3 et 24. Au moins, le lecteur aurait pu voir immédiatement qu'il y avait 118 illustrations. Il eût été aussi satisfait - au lieu de devoir le rechercher lui-même, car après tout, il n'est pas l'auteur du livre - de trouver une table des matières de ces planches au lieu d'être forcé de les feuilleter les unes après les autres pour les inventorier. Puisque Robin lui refuse ce service - son nouveau rôle dans la cella académique le gonfle au point qu'il s'arroge gracieusement la loi du "De minimis non curat praetor" - le lecteur doit faire cet inventaire lui-même. En voici une brève présentation schématique suivie de quelques

remarques.

	Textes	Nombre de photos directes	Nombre d'estampages photographies	Nombre de fac-similés	Pas de photos	Nombre de photos	Nombre de dessins archéologiques
Haram	56	30	3	3	28	3	2
ʾInabbaʾ	4	6				2	
al-Kâfir	32	32					
Kamna	22	18			7	10	2
al-Ḥarâsif	3	3				2	
	117	89	3	3	35	17	4

Note - La différence de certains chiffres entre ce tableau et celui de plus haut, p.47, vient de ce que le compte des photos de p. 47 se fonde sur les textes, alors que, ci-dessus, c'est sur le nombre de photos. P.ex., pour les estampages, le "2" de p. 47 se rapporte à Haram 30 et 55, et le "3" de ci-dessus aux 3 photos des pl. 9a, b et 20c.

Les 8 dessins comprennent les sujets suivants:

1 carte, pl. 1;
1 plan de site, p. 40;
1 plan de temple, pl. 44;
1 petit croquis de motifs décoratifs, pl. 22;
1 grand croquis de motis decoratifs dont la pl. 22 ne reproduit qu'une toute petite partie, pl. 60; aucun renvoi de l'un à l'autre et au CIASA, I, pp.254-255:dessins partiellement identiques;
3 fac-similés de textes, pl. 19a,b et 20b.

La condition et l'utilisation des photos de texte par Robin ont déjà été présentées plus haut, pp. 40 et 46-48. Il ne reste plus ici qu'à formuler quelques remarques sur les autres illustrations.

La toute 1ère illustration, pl. 1, une "Carte du Jawf antique" fut élaborée par "Madeleine SINTES". J'y reviendrai plus longuement dans mon compte rendu (à paraître) du volume de J.-F. Breton sur Les fortifications ..., publication tout aussi chauvine que celle de Robin; cf. plus haut, les remarques des pages 52-53.

Les 2 dessins se rapportant à Haram, pl. 22 et 60, sont décrits comme "le décor du linteau de la porte", et "le décor de la porte", les 2 appartenant à "Haram, temple des 'Filles de ʿĀd' (dessin: Rémy Audouin)". Toujours dans la ligne du je-m'en-foutisme "Robinien", le lecteur doit trouver par lui-même que la pl. 22 est en fait un agrandissement de 20,7 x 4,75 cm du centre supérieur de la pl. 60 de 36,8 x 22,9 cm, où la section agrandie mesure 15,9 x 3,45, et dont le dessin original remonte à 1981. Le lecteur n'est pas non plus informé des dimensions exactes de cette porte et surtout si, oui ou non, les dessins sont faits à l'échelle. Si le lecteur comprend aisément que R. Audouin ait désiré vivement voir son dessin publié sans plus tarder, il doit cependant à

la vérité d'admettre que sa publication eût été tout naturellement à sa place dans une publication d'archéologie ou d'art, comme, p.ex., Syria qui ne refuse rien aux membres du CNRS. Inséré dans le pamphlet de Robin, ce dessin n'est qu'un cheveu de couleur douteuse et de valeur nulle dans la soupe "Robinienne", car il NE CONTRIBUE EN RIEN à la compréhension des textes de Haram.

La double critique précédente s'applique en toute logique à chacun des 2 plans des pl. 40 et 44 sur Kamna. Ces 4 illustrations ne sont même pas des bouche-trous puiqu'il n'y avait pas de trous à boucher; elles sont simplement des hors-d'oeuvre nettement superflus, des espèces de fioritures très blafardes dans la bouillabaisse (cf. ici, p.143) incolore du pamphlet.

13 - Conclusion.

La volonté de ne retenir dans ce compte rendu que des questions requérant un minimum de recherche se justifie par la préoccupation d'englober un grand nombre de points disparates qui, par leur diversité et leur variété, ont plus de chance de représenter plus adéquatement les divers aspects du pamphlet. Toute reconstitution historique exige, en effet, de longs développements, surtout lorsque les points de départ sont déjà faussés par des réflexions et des hypothèses préconçues ou inexactes. C'est au court de ces nombreux sondages que l'idée se fit plus pressante d'inclure les petites questions qui étaient déjà apparues à la lecture comme des microcosmes éloquents et significatifs de la méthode "Robinienne". Ces 28 courtes études, 10 sur le déchiffrement, 6 sur la traduction et 11 sur les textes, auxquelles on doit ajouter l'anépigraphe al-Kâfir 33, servent de toile de fond à l'ensemble des critiques qui permettent de les mieux comprendre en révélant les réactions concrètes de l'auteur pataugeant dans des problèmes précis de textes très simples. La faillite de l'auteur dans ces cas bien délimités de petits textes sape le climat de crédibilité des esquisses historiques qui ne sont que des synthèses basées sur une manipulation d'hypothèses hâtives, presque constamment étayées de considérations injustifiées ou imaginatives. Ce que rappelle bien le quolibet universitaire: 1 thèse, des hypothèses, 1 synthèse, 1 foutaise. En bonne logique, le lecteur se doit de constater que Robin ne paraît pas se rendre compte que, quoi qu'il en pense ou puisse affirmer, il ne fait qu'emprunter dans son genre particulier l'ornière fantasmagorique ouverte jadis par ce génie des synthèses ébouriffantes à grand spectacle, qu'était Jacqueline Pirenne. De plus, les très nombreuses répétitions qui ne sont que rarement signalées par des renvois, de même que l'absence d'un index général détaillé des termes sud-arabes des textes (cf. plus haut, pp. 91-92) ainsi que le manque de liste des abréviations (cf. plus haut, pp. 92-93) rendent le pamphlet inutilisable en rejetant sur le lecteur la corvée de compilations que l'auteur se devait de faire pour tous. Il ne l'a pas fait pour des raisons qui, pour être tenues cachées, ne se laissent

pas moins deviner et n'en sont pas moins condammnables. En un mot, l'auteur porte la responsabilité des graves déficiences manifestes de son pamphlet, des entraves qu'il impose au chercheur ainsi que de leurs conséquences négatives sur les travaux de celui qui serait forcé de se servir de sa production littéraire qu'il limitera nécessairement à la documentation photographique, pour autant qu'elle soit utlisable.

C'est à la lumière des réflexions précédentes basées sur un amoncellement massif de faits et de remarques de tout genre, remplissant les 59 pages précédentes que le lecteur pourra à son aise et en tout objectivité jauger à sa véritable valeur l'appréciation du recenseur du pamphlet, telle qu'elle peut se lire dans la "Préface" (p. 2), savoir "sa connaissance intime des antiquités sudarabiques, et avec une grande prudence dans son jugement" (ll. 13-14).

Il y a presque 40 ans que le lecteur fut confronté à un cas semblable de dichotomie entre la thèse de J. Pirenne et son rapporteur de l'Académie Royale Flamande de Belgique (cf. plus haut, p. 44). Inutile de s'appesantir sur le verdict que l'histoire des dernières années n'a cessé de faire tomber en faveur de mon opposition totale, manifestée dès le 1er jour et répétée à chaque occasion possible et cela nonobstant l'utilisation constante, parfois timide mais toujours réelle, des sudarabisants, nonobstant les efforts déployés par certains auteurs, tels J. Ryckmans et son suivant, C. Robin (cf. plus haut, p. 14) et surtout Robin dans son pamphlet pour insuffler un regain de vie, fugace comme l'éclair, aux classifications, toutes imaginaires, et aux dates, toutes saugrenues et mort-nées de Pirenne (cf. plus haut, p. 44). Encore, convient-il de ne pas méjuger les intentions de Robin, car ses emprunts massifs et ses actes de dépendance des catégories et des dates de Pirenne pourraient avoir été motivés par un but de politicaillerie. Maintenant que J. Pirenne a été prématurément liquidée de l'arène sud-arabe et parisienne et surtout - hélas! - a disparu de la scène terrestre, les publications futures de Robin permettront au lecteur de replacer le pamphlet dans une perspective qui rendra possible une évaluation exacte de la réalité. Mais Robin et compagnie n'ont pas tardé à marquer leur palinodie, comme en témoigne le titre de l'ouvrage récent de J.-P. Breton (cf. plus haut, p. 94) avec la mention du "7e siècle avant notre ère" où il ne fait d'ailleurs que suivre le mot d'ordre de Robin; cf. ici, p. 98. Le volume édité par J.-P. Breton en 1992, Fouilles de Shabwa. II. Rapports préliminaires (déjà paru en articles en 1991) ne souffle mot de la question. Tous ces auteurs qui ont en commun la volonté chauvine de taire à tout prix le fait historique rappelé plus haut (pp. 44-45) que j'ai été le premier et le seul à rejeter d'emblée dès le 1er jour l'échafaudage historico-paléographique de J. Pirenne, tout fait de papier mâché. Le lecteur fut derechef soumis tout récemment à ce genre de moquerie avec l'appréciation dithyrambique

par G. Gnoli du volume d'A. de Maigret ed., The Sabaean Archaeological Complex in the Wādī Yalā ... (Rome, 1988), une appréciation que mes Miscellanées ..., XVII (1989), p. 93, caractérisent bien simplement de "out of touch with the publication itself".

En guise de synthèse finale des "Robineries" dénoncées tout au long des pages précédentes, il paraît utile de présenter au lecteur ce qui pourrait s'appeler la quintessence du quant-à-moi "Robinien", savoir:

1 - la contradiction écoeurante de publier des textes sans photo, ce que sa thèse doctorale elle-même de 1977 et 1982 avait persiflé hargneusement;
2 - l'utilisation de ses photos rarement convenables, normalement déficientes et parfois même inutiles;
3 - le pullulement sursaturé de la vermine de ses gadgets typographiques balafrant tous les mots sans répit et ne faisant grâce à aucun;
4 - sa paléographie de primate se limitant à quelques passe-partout généraux et imprécis;
5 - ses argumentations hâtives, tendancieuses et toujours subjectives;
6 - son choix exclusif de ce qui lui plaît et, parallèlement, son rejet de tout ce qui lui déplaît dans le dépotoir de l'Académie des Inscripions et Belles-Lettres et, selon toute apparence, avec l'assentiment de celle-ci;
7 - son pan-classicisme arabe sectaire, aveugle et total; et enfin
8 - la publication désinvolte de textes (cf. déjà plus haut, pp. 40, 47-50, etc, etc).

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Note additionnelle aux pp. 51-52. - Le nom de ville nšn devient dans l'orthographie "Robinienne" "nsʾn et, en traduction," soit "Nashan" ou "Nashan" ou encore "Nashshān" dans The New Encyclopaedia of Islam, IX (1995), p. 91A, où un autre nom de ville, nšqm, est encore plus maltraité puisqu'il devient "Nashkum", avec la mimation toujours condamnée à rester "un soffite permanent ancré au plafond."

Note additionnelle à ᶜbd/ʾdm (qui sert de pluriel au 1er) de p. 67. - Robin réitère une fois de plus sa conviction moyenâgeuse de ʾdm en maintenant sa traduction en "clients" à la p. 91B de l'article cité 5 lignes plus haut. Quel anachronisme!

Note additionnelle au "pseudo-sabéen" de Robin, p. 92. - Son article cité plus haut ne parle que de "Sabaean" et plus aucune mention n'est faite de son "pseudo-sabéen, bien qu'il renvoie à cet article, "1992, 31ff". D'ailleurs, la distinction régionale en cause a été signalée bien avant lui par A.F.L. Beeston en 1962, qu'il utilise.

Note additionnelle à la p. 96, bas. - Toujours dans le même article cité plus haut, la volte-face de Robin est bel et bien consommée sans tambour ni trompette: il parle sans sourciller du royaume de Našān "in the 8th century B.C." (p. 91A). C'est à peine croyable!

III

The Thamudic Inscriptions of Wâdî ʾAlâb (Saudi Arabia) published by A.R al-Ansari.

The Thamudic inscriptions of Wadi ʾAlâb were seen by M.I. al-Ḥâzmî in December 23-24, 1968. Some of them were published in Arabic by A.R. al-Ansari in the Bulletin of the Faculty of Arts, University of Riyad, 1 (1390/1970), pp. 113-120, facsimiles of the edited texts in p. 121, and 4 photos in pp. 122-124. All the graffiti apparently are vertical or aslant to the right, to be read from top to bottom. My study of those graffiti is long overdue. But I had hoped for working on photos clearer than nos.2 and 3 which contain 7 out of the 10 edited graffiti.

The characteristics of al-Ansari's treatment of the texts may be presented as follows.

1 - The editor's scientific documentation appears to be limited to a triad of authors, viz F.V. Winnett (1937), E. Littmann (1940) and A. van den Branden (1950, 1956,1960 and 1966). Beside them there is only an all-encompassing Rubᶜ al-Ḫâlî with 2 cases,viz Jaussen-Savignac (1914; 2 references in pp. 117 and 120) and his own unpublished thesis on Liḥyanite personal names (1966; reference in p. 116 on ḥbbt). But, what is the pertinence of an unpublished 1966 thesis on Liḥyanite personal names to Thamudic ḥbbt which has not yet been found in Liḥyanite and derives from ḥbb, a root attested all through pre-Islamic Arabic?

2 - The preceding fundamental dependence from the above-listed antiquated triad does not, however, explain the reason or reasons why al-Ansari developed his special brand of understanding and approach to Thamudic. His attitude seems probably to be the "ab uno disce omnes" principle by which the characteristic of several texts is forcibly applied to the others of the same kind. This process is exemplified by the 2 following cases.

a - Eight of 10 texts reproduced in p. 121 begin with dn (cf. my tracing below). Therefore, al-Ansari's apparently simplistic approach to the texts suggested to him that bn introducing ʾAlâb 9 should also be read as dn (p. 119), a reading that he himself condemns since he reads the beginning of ʾAlâb 6 as bn (p. 118).

b - Seven of the same 10 texts contain each a single personal name. Here again, his apparent simplism suggested to him that the 3 remaining texts, ʾAlâb 2, 3 and 4,were also composed of personal names exclusively.

Unfortunately, the 2 preceding simplistic applications forced al-Ansari to resort to some novel subterfuges.

For the background of his approach to Thamudic, al-Ansari dug out and strove to rejuvenate a characteristic of the above-listed triad (cf. above, p. 99), viz the long-buried theory of initial dn being "This" instead of "Has perished" (cf. my Thamudic Studies, Washington, 1967, pp. 3-32), and of the initial bn being "Son of" instead of "Has stopped;" cf. l.c., p. 39, with the following statement: "There is no Tham[udic] text which starts with 'Son of'."

al-Ansari reads ʾAlâb 2 as "dn dn ..." (p. 116) with dn = "hadâ" dittoed twice one after the other and introducing 2 personal names in asyndetical succession! What a feat! Yet, al-Ansari outdoes his preceding performance in ʾAlâb 3, where the initial dn = "hadâ" is given the value of the masculine dual "hadâni", and his legerdemain forcibly suggests the restoration of 2 nonexistent words, "bn" ("son of") and "bnt" ("daughter of") which, in turn, will usher in the 2 personal names demanded by his dual "hadâni." The restitution of such hinges and the like was already denounced in BiOr, 24 (1967), p. 281A, note 9, my Thamudic Studies, p. 2, and repeated in my Miscellanées..., XV (1987), p. 182. Incidentally, al-Ansari restores bnt daughter of between "ḫbbt" and "lhmt." Such a theory comes from the reading of Thamudic "ḫbbt" as Arabic ḥabîbat, the feminine of ḥabîb loved, beloved. But, such interpretation remains unprovable since ḫbbt could be ḫubbat love, or ḫabbat core. Note also ḫubbâbat, the 1st element of a personal name in F. Wüstenfeld, Jacut's geographishes Wörterbuch, Leipzig, VI (1870-1871), p. 374, (1870-1871), p. 374, and Safaitic ḫbbt is masculine in CIS 897, 5114, 5119 and Ox 42 with ḫbbt bn each. Yet, both CIS and G.W. Oxtoby vocalize the masculine name as "Ḥabîbat" as if it were the feminine of ḥabîb, as had done G. Ryckmans (cf. Les noms propres sud-sémitiques, Louvain, I [1934], p. 86B). The name ḫbbt is also feminine in Safaitic Damas 5537 and Sabaean Ja 509, and also a family name in Sabaean Ja 701/2.

3 - Inasmuch as they can be seen on the photos, al-Ansari's facsimiles of p. 121 are only remote approximations of the originals; e.g., the tracing of the letter 𐩢 = m of ʾAlâb 9 is a serious distortion of 𐩣 = ṯ.

4 - The photos are of poor quality; too many graffiti are practically illegible, and among them al-Ansari chose 10.

Photo 1 (p. 122, top) allows the reading of ʾAlâb 1 that is standing between 2 other graffiti unread in the edition.

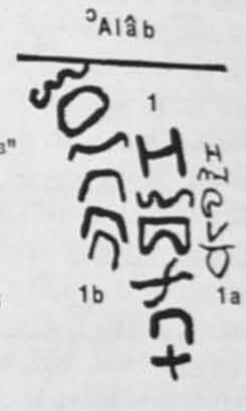
Photo 2 (p. 122, bottom) allows the reader to see ʾAlâb 2, but half of the letters remain illegible.

Photo 3 (p. 123) which contains, according to the caption, ʾAlâb 3, 4, 5, 6, 7 and 8, is practically useless. It is, however, possible to detect that ʾAlâb 4 is located 6 cm to the right of the left edge of the photo.

Photo 4 (p. 124) is the best of the 4, with ʾAlâb 9 and 10 in the extreme right section between the right edge of the photo and 2 standing ibexes. To the left of these animals there is a group of 4 texts unread by al-Ansari; yet they are quite readable. This is very surprising because ʾAlâb 3-8, although unreadable in photo 3, were deciphered apparently without any difficulty. The reasons why al-Ansari did not read ʾAlâb 1a, b, 11, 12, 13 and 14 are difficult to fathom in at least 4 cases. ʾAlâb 1a and 13 have each only one single personal name as his no.1; ʾAlâb 13 is introduced by bn as his no.6; and he could have applied the scheme used in his no.3 to ʾAlâb 1b and 12. The reason for omitting ʾAlâb 11 and 14 may be the fact that they do not start with either bn or dn, and the mere restoration of the hinges bn and bnt (cf. above, p. 100) would have been of very little help toward an acceptable reading of the 2 graffiti.

Here follows my study of the 9 texts readable in the published photos along with their tracings made from the photos.

ʾAlâb 1 (p. 115, and photo in p. 122, top):



↓ dn mhbt Has perished Muhbit.

Same reading of the letters as in the edition. - dn, cf. above, p. 100. - mhbt, cf. Arabic muhbit "still, motionless" (cf. E.W. Lane, p. 693B).

ʾAlâb 1a (photo in p. 122, top): a short text composed of much smaller letters to the right of, and parallel to, the 1st 4 letters of the preceding graffito; unread in the edition.

↓ dn mrd Has perished Mârid.

mrd, e.g., Hu 142b.

ʾAlâb 1b (photo in p. 122, top): a text composed of letters as large as in ʾAlâb 1, forming a concaved line, and to the left of, and parallel to, the same text; the initial letter was cut off by the photo; unread in the edition.

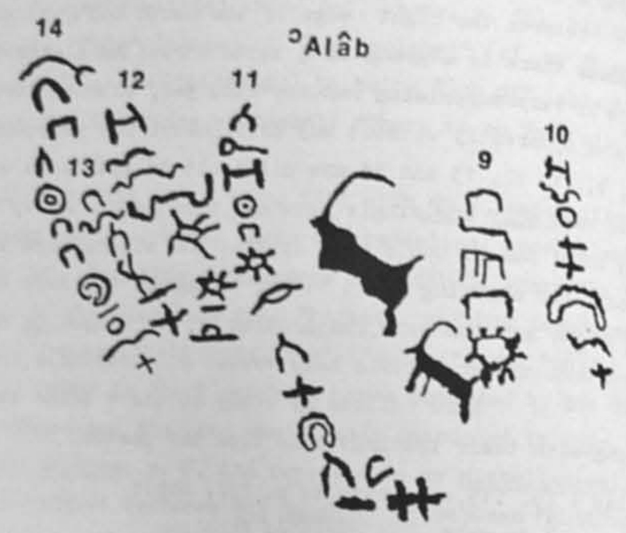
↘ [d]n ʿfr rb [Has per]ished ʿUfar [who was] a master.

ʿfr, cf. Safaitic JaS 194; cf. also ʿfrw of HaLi 137, and ʿfry of Rho 20. - rb, e.g., JsaT 243.

The 6 texts of photo 3, ʾAlâb 9-14, are presented below (next page). ʾAlâb 9 and 10 to the right of 2 ibexes looking leftward are almost vertical and slightly aslant to the right, respectively. Note that the forelegs of the upper ibex are not visible on the photo and are therefore missing in my tracing. ʾAlâb 11-14, unread in the edition, are very aslant to the right, parallel to each other and to the left of the 2

ibexes.

ʾAlâb 9 (p. 119): immediately to the right of the 2 ibexes, and slightly aslant to the right.



bn ṯbš

Has stopped [here] Ṯâbiš.

For the editor's misreading of both b and ṯ, cf. above, respectively, pp. 99 and 100. - bn, a well-known verb, e.g., JaT 9/3, 38g, 130, etc. - ṯbš: the root ṯbš is known in some Arabic nouns, such as "ṯabš Homines"(cf. G.W. Freytag,III, p. 38A); cf. R. Dozy, II, p. 21A, for other derivations. The root ṯbš is still used in Near-East Arabic; cf. ṯabaša "claquer (la porte); dormir d'un sommeil profond" (cf. C. Denizeau, Dictionnaire des parlers arabes de Syrie, Liban, Palestine, Paris, 1960, p. 323, and also A. Barthélemy, Dictionnaire arabe-français. Dialectes de Syrie: Alep, Damas, Liban, Jérusalem, livraison 3 [1942], p. 469). The root ṯbš is also used in maghrebian Arabic; cf. ṯbš, 2nd form "dresser les oreilles, cheval" (cf. M. Beaussier, Dictionnaire pratique arabe-français, Alger, 1931, p. 597B, and A. Nicolas, Dictionnaire arabe-français, Tunis, 1938, p. 159A).

ʾAlâb 10(p.119):to the right of, and parallel to, the preceding graffito, and aslant to the right.

dn ʿttmnt Has perished ʿAttmanat.

The 1st element of this new theophoric name is found in Naveh Negev 1 and also as a verb in Hu 407.

ʾAlâb 11: immediately to the left of the 2 ibexes, and aslant to the right.

rt 2

sy dgbš qs tmsl 1

1 Sawî, he of (the family) Gâbiš, has calumniated Tamsal

2 [who was] old.

sy, cf. Safaitic WiTh 16. - gbš, cf. Arabic jabaša "Rasit crines" (cf. G.W. Freytag, I, p. 241A) and jabš "nom d'un jabše 'galets, cailloux...'"(cf. C. Denizeau, l.c., p. 74).- qs, cf. feminine qst of JaT 88; and my Miscellanées ..., XIV (1985),p. 35; pp. 17-35

deal with the whole text that E.A. Knauf published as "A South Safaitic Alphabet": - tmsl, cf. Safaitic msl as a personal name in Wi 527 and the noun "place where the water runs" in Ox 2. - rt, cf. Arabic ratta "to be old, and worn out, weak, etc (man)" (cf. E.W. Lane, pp. 1029C-1030A).

ʾAlâb 12: immediately to the left of ʾAlâb 11, and almost parallel to it.

dn nfšš ld Has perished Nafšaš [who] had rebelled.

nfšš, a qatlal reduplication) nominal derivation; cf. nfš of Dut 299d. - ld, e.g., Qasrina 3.

ʾAlâb 13: immediately to the last 2/3 of the left of, and parallel to, ʾAlâb 12.

bn shlt Has stopped [here] Sahlat.

shlt, cf. shl, e.g., in HaLi 345. G.L. Harding's commentary on HaLi 394 states that "there is some doubt about the last name and its connection with this text" (cf. G.L. Harding - E. Littmann, Some Thamudic Inscriptions ..., Leiden, 1952, p. 38). Harding's facsimile of pl. 18 is particularly freakish as proven by the photo of pl. 17, upper left part, especially the letter s which is drawn as d! The last 5 letters form a short separate text, HaLi 394a because (1) the photo does not indicate why the engraver would have been justified in separating the 2 sections of his text; (2) the extreme left letter most certainly is l, and cannot be "b" as in the edition. HaLi 394a may be read as follows:

ln shl Was gentle Suhhal. - ln, Safaitic WH 3233. Hu 772/2 where A. van den Branden reads ln f- to show gentleness to (cf. Inscriptions thamoudéennes, Louvain, 1950, pp. 164-165) needs to be treated again because the text has been so far understood as an invocation to the god Ruḍâ, "h[r]ḍw," a reading already found in J. Halévy's copy (cf. Nouvel essai sur les inscriptions proto-arabes, Paris, 1903, p. 75, no. 553). This restoration is contradicted by C. Huber's copy (cf. Journal de voyage en Arabie (1883-1884), Paris, 1891, p. 526, no. 56/2) which shows the beginning of the text to read hdw, as in Halévy's copy without his restored "r." The text may be read as follows: hḍw ln fzmn hḥqm

Hadâw was gentle, and Haḥaqm was crippled.

hḍw, cf. Arabic dâw^{in} "lean, emaciated" (cf. E.W. Lane, p. 1811B). - f, e.g., JaT 20 a. - zmn, Hu 280. - hḥqm, cf. Arabic ḥaqm "pigeon or similar bird" (cf. G.W. Freytag, I, p. 408); not mentioned in any of the 5 Arabic dictionaries listed after Freytag in the commentary on ʾAlâb 9 (cf. above, p. 102). ʾḥqm and ʾḥlm are possible emendations of thqm of WH 488 suggested by the editor (cf. F.V. Winnett - G.L. Harding, Inscriptions from Fifty Safaitic Cairns, Toronto, 1978, pp. 120 and 563A; and this "thqm" is listed among the "rare consonantic equivalences" in my Miscellanées ..., XIII (1983), pp. 183 and 244-245: "permutation t - f"). The great difficulty of emending a text as the present

is to determine which letter to chose and whether or not only one letter must be emended; viz ṯ to ʾ, ġ and ṛ; ḫ to s; and q to w (l seems to be excluded by the width of the letter itself).

A 4th suggestion is hereby presented in 3 cases where lt introduces the text.

(1) The beginning of WH 488 may be read as lt ḥqm Has stayed [here] Ḥaqm. - lt = ltt, e.g., JaT 103f. In LP 337, the editor, E. Littmann (cf. Safaïtic Inscriptions, Leyden, 1943, pp. 85 and 322) mentions also the possible derivation "lāth, i.e. 'he sought refuge'." This alternative is unacceptable because the idea of "to seek refuge" does not belong to the root lwṯ (it is indeed not listed in Wörterbuch der classischen arabischen Sprache, Wiesbaden, 1983, II/1, p. 187) since E.W. Lane points out (p. 2678A) that "the ṯ here is a substitute for the d of lāda"= the root lwd.

(2) The beginning of WH 3105 may be read as follows:

lt ḥr Has stayed [here] Ḥurr. - ḥr, e.g., JaS 140a.

(3) The beginning of HaLi 128 may be read as follows:

lt ḥl Has stayed [here] Ḥall. - ḥl, Euting (562), and Safaitic, e.g., Wi 569a. ʾAlāb 14: immediately to the left of, and parallel to, ʾAlāb 13.

hrb sġr bmlᶜft

Has run away Saġīr, son of Malᶜafat.

hrb, e.g., JsaT 390. - sġr, cf. ʾsġr, e.g., in WiTa 17A. - mlᶜft, cf. Safaitic lᶜf, e.g. e.g., WH 1891, and Thamudic lᶜfl of Hu 150.

August 26, 1993.

IV

The J.F. Healey - G.R. Smith tandem's study of Nabataean JsaN 17 and Thamudic JsaT 1.

The following presentation dissects the shameful work on JsaN 17 by the tandem J.F. Healey and G. Rex Smith in Atlal, 12 (1410/1989), pp. 77-84 and pl. 46A-B, under the title of "JAUSSEN-SAVIGNAC 17 - THE EARLIEST DATED ARABIC DOCUMENT (A.D. 267)." Healey strained every nerve to demonstrate that this Nabataean text actually is Arabic. All too happy to endorse Healey's pronunciamento without formulating the slightest objection, Smith is free to apply the partisanship of his pan-classical Arabic fanaticism. The presentation of the views of the Healey-Smith tandem is complemented by some notes on the Nabataean text JsaN 17 according to M. O'Connor's statements published in JNES, 47 (1986), pp. 221-227, and these notes are followed by 2 short addenda devoted to J. Naveh and E.A. Knauf.

I - The Healey-Smith tandem.

A - General remarks.

1 - Healey refers from the start to "plate [sic] 46-47" (p. 77, l.28). In fact, each plate is labeled "Plate 46 لوحة ٤٦," a label which, for the purpose of accurate reference, becomes henceforth pl. 46A and B, respectively.

2 - The joint paper makes use of an incomplete siglum of the text involved, "Jaussen-Savignac 17," which is repeated in Arabic in the running heads and captions of pl. 46A and B; it is abbreviated as "JS 17" in p. 3, the running heads of pp. 77-84, pl. 46A and B, and the captions of these 2 plates. It has been known for about 85 years that J.A. Jaussen - M.R. Savignac, Mission archéologique en Arabie (mars-mai 1907), Paris, I (1909), contains inscriptions distributed on the basis of their language, viz Nabataean, Liḥyanite and Thamudic; the texts written in Minaean, Arabic and Greek are not considered here because of their small number. The siglum "JS 17" does not indicate the name of the language under which it was published and, therefore, it necessarily refers to 1 of the 3 different categories listed above.

3 - Although the introduction of pp. 77-80 is signed by Healey, Smith cannot escape from being accountable for its contents because he bases his contribution on Healey's work; he never explicitely repudiates it or even utters any doubt about it. Quite the contrary and above all, he endorses the title which summarizes and introduces his own contribution.

4 - This Healey's production is marred by incomplete (cf. already above, the label of the 2 plates) and confusing statements. Another example of such a case is found in p. 77, ll. 22-23: "The inscription itself is bilingual in that there is a short Thamudic summary written vertically to the right."

In the 21 preceding lines Healey speaks only of "JS 17" = JsaN 17 (cf. Jaussen-Savignac, l.c., pp. 172-178, photo of the squeeze and the facsimile in Planches, pl. IX and XXV, respectively). The Thamudic text is published somewhere else by Jaussen-Savignac, l.c., pp. 271-272, as no. 1 of their "Graffites tamoudéens;" thus, JsaT 1 = Hut 772 (cf. my in pl. 36A) = Hu 562 (cf. my Thamudic Studies, Washington, 1967, p. 15, and my Miscellanées d'ancient arabe, Washington, V [1974], p. 118A and J. Euting's facsimile nées ..., l.c., p. 123C). Furthermore, the Thamudic text can obviously not be considered as "a short ... summary" of the long Nabataean inscription. To join them together in one inscription, as Healey does when he speaks of "The inscription itself" (cf. also the expression "la bilingue" by J.T. Milik - J. Starcky in F.V. Winnett - W.L. Reed, Ancient Records from North Arabia, Toronto, 1970, p. 154, no. 91, or "la bilingue thamoudéo-nabatéenne" by J. Starcky in "Pétra et la Nabatène" in SDB, VII [1966], col. 932, copied by E. Gruendler, cf. next page) is all the more inconsiderate and irrealistic that JsaN 18 also belongs to the same group as the 3rd panel of a triptych because it contains the name of the master-mason hn²w and the mention of his anonymous helpers, of some other masons and of the carver of ktb² d² this writing (JsaN 18/4). Of course, for the carver there is only one job, one writing. Healey is aware of JsaN 18 which is, in his view, "two related graffiti on the same rock" (p. 77, ll. 28-29). It is senseless and irresponsible to so characterize a single text composed of 4 lines divided in 2 sections, the 2nd section being introduced by w and. In his cryptic idiom, Healey states that "discussion of these is left to fuller publication" (p. 77, l.29) of Milik-Starcky.

5 - The photo of pl. 46A has cut off the last 2 letters of l.7, a fact that neither coauthor ever mention ... as if neither had even noticed it or cared about it. However, as the actual end of l.7 is crucial in the argumentation on the end of l.8, and it is precisely that end that is left out, the reader cannot but wonder whether the cut actually was accidental, and if so, whether it was not conveniently maintained in order to help the tandem's argumentation on the end of l. 8.

6 - The value of the scale visible on the photo of pl. 46A is nowhere mentioned or explained; and it cannot be guessed at by the reader because none of the coauthors states that, according to Jaussen-Savignac in RB, 1908, p. 241, the Nabataean text is 70 cm wide, and the letter height about 8 cm. The scale of pl. 46A represents 40 (4 x 10) cm. In the editors' Mission ..., correct "1^{m},70" (cf. I, p. 172) to "0^{m},70." For the scale, cf. my Miscellanées ..., XVIII (1995), p. 148.

7 - According to the title of Healey-Smith's paper (p. 77) and p. 82, the date of JsaN 17 is "A.D. 267;" but Healey gives "267/8 A.D." in Aram, 2 (1990), p. 93. J.A. Jaussen - M.R. Savignac, l.c. p. 174, had already given "162 + 106 ap. J.-C.," "thus

268; and B. Gruendler in The Development of the Arabic Scripts, Atlanta, 1993, p. 108, gives "268/9 CE."

8 - Both coauthors fail to use diacritical signs to distinguish some letters, thus making their readings erroneous, viz 7 and 2 diacritical signs must be restored to the Nabataean and Thamudic texts, respectively.

9 - Healey's information aimed at deceiving the reader is that some "refinements in the readings were suggested by J.T. Milik and J. Starcky" (p. 77, ll. 24-25) with reference in note 5 (p. 83) to Winnett-Reed's 1970 book (cf. above, p. 106). Milik-Starcky's contribution is mentioned twice by Healey,

(1) p. 79, ll. 3-4: "Milik and Starcky . agree on mn yqbr." This is J.-B. Chabot's reading already mentioned by Jaussen-Savignac (cf. l.c., I, p. 481); there is thus no question of Milik-Starcky's "refinements;" and

(2) p. 79, ll. 5-6: "it has been more usual to read w²ly mnh" with note 16 (p. 83) referring to "Milik and Starcky, op. cit., 155." This is erroneous because the Milik-Starcky tandem endorses Chabot's reading of w²ʿly mnh. Here again, there is no question of Milik-Starcky's "refinements." Incidentally, Milik-Starcky claim that Jaussen-Savignac's original reading, d² ʿly mnh, was "reprise par distraction par Cantineau, Le nabatéen, II, pp. 38sq et passim." The precision "par distraction" is erroneous because, after mentioning Chabot's reading, Cantineau adds that "JAUSSEN maintient sa lecture, qui est plus satisfaisante, sauf l'emploi de yʿyr à côté de yn² l.7 avec un sens presque identique" (cf. II, p. 39). Finally, the 3rd discrepancy between Jaussen-Savignac's text and Milik-Starcky's is this: "à la fin de la l. 8, lire d ou dy et non pas w de Savignac" (cf. l.c., p. 155; cf. immediately below for further development). The reader remains glabbergasted by Healey's voluntary deceptive use of the word "refinements" as the 1st 2 different readings of Milik-Starcky mentioned by Healey were mere endorsements of Chabot's readings, and the 3rd one is a brand new reading that Healey does NOT EVEN acknowledge although M. O'Connor endorsed it openly and unquestionably (p. 225, ll.12, 26, 28 and fn 72, p. 226, ll. 2, and p. 227, ll. 3 and 12); for O'Connor, cf. below, pp. 129-142.

10 - Another Healey-Smith's loathsome blunder concerns the end of l.8, which the editors transcribe as "w" and understand, as M. Lidzbarski, as the initial letter of the next word reported by the engraver to the next line. The letter is extravagantly called an "error" (p. 78, l. 40) by Healey who states that "there was no reason why the mason could not have continued on the same line" (l. 41). This is untrue. And he adds the following restriction: "unless he could not reach further, bearing in mind that the text is written on a sheer rock-face" (ll. 41-42). Untrue again! Even a child

sees that the preceding l.7 is 2 letters longer than l.8: "w" of l.8 is just below and slightly to the right of r of gbrw. The letter is treated differently by Healey and Smith who erroneously use parentheses (p. 78, l. 8) and brackets (p. 80, l. 18), respectively! Even Smith practically eliminates the letter, viz no mention of it is to be found in his commentary (p. 81), his transcription of the text in Arabic (p.82) and his translation (p. 82). It is hard to believe it, but, Healey, who refers explicitely to Milik-Starcky's work on the text no less than 5 times (pp. 77, ll. 24-25; 79, l. 3 and notes 5, 6 and 16 on p. 83), fails to mention and to utilize their first and only new reading, viz. "à la fin de la l. 8, lire d ou dy et non pas w de Savignac" (p.155, l. 1; cf. already above, p. 107). Yet, it is most important because it clarifies the value of wld (l.9) and the antecedent of -h of wldh (l.9; cf. below p. 126). For the last remarks on the present case, cf. below, pp. 113-114.

11 - Still another error of both Healey and Smith is their failure to recognize that W.W. Müller, another pan-classical Arabic fanatic, had already stated in 1982 (cf. W. Fischer, Grundgriss der arabischen Philologie, I, Sprachwissenschaft, Wiesbaden, p. 33) that the text contains too many arabisms to still be classified as Nabataean. However, Müller's arabisms are less numerous than those listed by J. Cantineau, Le nabatéen; e.g., the l'n perfect understood as a expression of wish or prayer (cf. below, pp. 118-119) is missing in his summary. But, he also notes that there is inconsistency in the transcription of the proper names concerning the -w ending, viz rgws has no -w ending, but the 3 other names are spelled as in Nabataean, e.g., according to him, "ḫrtt;" cf. below pp. 123-124: ḫrtt is not Nabataean. But, ḫrtt is part of the overall problem since the last letter is -t (cf. J. Cantineau, II, pp. 164 and 166). It is inconsiderate to speak of "Inkonsequenz" here since the rule regulating the use of the -w ending has not yet been elucidated (cf. below, p. 138). Finally, Müller failed to acknowledge that 16 years earlier J. Starcky had declared (cf. "Pétra ...," col. 925, ll. 4-5) JsaN 17 to be "presque entièrement arabe" and should be left out from the study of Nabataean (cf. also below, p. 130). It is Müller at his best: where is the difference between his theory and Starcky's?

12 - The 2 fundamental errors of method.

a - It is in Atlal, 10 (1406/1986), p. 108, that Healey published this ominous statement:

"Jaussen and Savignac had the opportunity of seeing the inscriptions before they were subsequently damaged by the elements and by careless visitors. Some old photographs and drawings show that the tombs and inscriptions on them have suffered more damage in the last 100 years than in the previous 1800 years!" (ll. 27-30).

As if it was a characteristic of the new breed of researchers, Healey reacts exactly as C. Robin, viz he proclaims one of his seemingly basic principles of research

..., then he turns around and completely forgets about it (for C. Robin, cf above, pp. 46-47). Cf. also below, p. 113. The 2 coauthors' fundamental error of method is their failure to study the 2 squeezes made in the spring of 1907 and in 1907-1908 (I did not find any precision on the date of the 2nd squeeze) respectively (cf. Jaussen-Savignac, l.c., pp. XI, 172 and 481), which represent the condition of the rock surface almost 90 years ago. These 2 squeezes, therefore, nullify the value of any recent photo which can only register the effects of all deteriorating agents during all those long years. Yet, Healey justifies his new text by these 2 valueless considerations, viz

"Having been able to examine the inscription in situ and make use of new photographs" (p. 77, l. 27)

as if his eyes and/or a camera could by the simple magic of his personal hoepoe or jinn restore the text as it was at the beginning of this century.

To see how crookedly he draws the present forms of the letters shown on his photo — cf. already my statement on his "grossly distorting facsimile" in my Miscellanées ..., XVIII (1995), p. 148 — it suffices to compare the details of his preposterous sketch of pl. 46B with the photo of pl. 46A. It is evident that

(1) not a single letter of either text is traced accurately;

(2) the 2 texts run parallely contrary to what appears on the photo of pl. 46A (cf. also below, p. 137); and

(3) his distorted vision affects his reading of the spots relating to his so-called diacritical points and of the initial word of l.1 (cf. also below, pp. 112-114).

For the very same fundamental error of method repeated on the an-Nemārah inscription by A.F.L. Beeston, H.I. MacAdam and J.A. Bellamy; cf. below, pp. 117-118.

b - The 2 coauthors' 2nd fundamental error of method is to ignore that the author of JsaN 17 is Thamudaean according to the adjacent JsaT 1, and NOT AT ALL ARAB. It naturally never seems to have occurred to all those pan-classical Arabic fanatics to ask themselves why the so-called ARAB author (according to them) of JsaN 17 would have added a short text nearby in THAMUDIC? Why did he not write his small addition in Minaean since he was after all living in a very prosperous Minaean colony, instead of preferring the language and letters of a small, apparently powerless minority of bedouins? Ka'ab must have been a wealthy, a very wealthy man to afford to build such a tomb for his mother and to pay for the engraving not only of a lengthy funerary inscription, but also of JsaT 1 and JsaN 18. Therefore, he must have had daily and lengthy relations with the wealthy members of that Minaean colony. Why, for God's sake, would he select the language and the alphabet of those bedouins who could, at their very best, become his meanial servants? Furthermore, what proof do all those pan-classical Arabic

fanatics have that the Thamoudaean Ka^{c}ab could speak Arabic? The repeated use of the a-
rabic article can hardly prove that Ka^{c}ab could actually speak Arabic, and he did not
have to borrow the verb l^{c}n from outside since — but Healey, Smith or Müller obviously
did not wish to query about such a question — l^{c}n is known in Thamudic in the perfect
form in Hu 627 and 713 b/2, where the optative meaning is out of the question (cf. my
Miscellanées ..., XV [1987], pp. 178-180, and IV [1973], p. 137, note 79, respectively).
Finally, l^{c}n is also a personal name in JoDD 6. Cf. also below, pp. 117-118.

B - The Thamudic text (cf. already above, p. 106).

Healey-Smith's artifice is proven by their silence of all studies of the
text, including mine of 1967 which was based on the photos of 3 squeezes made in 1907
by Jaussen-Savignac beside that which is published in their volume (cf. I, l.c., p.271).
Furthermore, Smith's translations (p. 82, ll. 29-30), which are presented as being the
"best"!(l. 28), prove that his haughty, bragging pan-classical Arabic fanaticism is so
conclusive that it explains and justifies everything automatically. Yet, Smith is forced
to invent the word "grave" to make his 2 translations palatable. Furthermore, his 2nd
translation (l.30), viz "This [grave belongs to] R bint ^{c}A," beside inventing the word
"grave," is nonsensical because a corpse has no right and cannot, therefore, own any-
thing. Moreover, Smith's fn 39 (p. 84) limits its commentary to observing that Jaussen-
Savignac (l.c.) is "reading lqd!"where "d" must be corrected to d (cf. already above, p.
106, no. 8). The exclamation mark correctly indicates the shock of the reader seeing how
abusive and offensive Rex Smith is when ridiculing Jaussen-Savignac's titanic work done
at a time when he was nowhere and North-Arabic palaeography and research were still in
the cradle. Summarily, both Healey's and Smith's stands were already proven erroneous
more than a quarter of century ago by my Thamudic Studies (pp. 4-52) where dn is demons-
trated to be either a verb, to perish (e.g., in Cadès 4, 9, Dghty 16/8, etc in p. 10,
HaLi 100 = 101 in p. 32, and below, p.143, NaT 2-5) or a noun translated as tomb in the
present case (p. 15) and in Gheita a, b (p. 31) and as burial urn in Goshen 350 a (pp.
31-32).

C - The Nabataean text.

1 - Preliminary remarks.

a - The 2 transcriptions of the text. - Healey's transliteration (p. 78) is made "on
basis of the traditional Nabataean alphabetic values" (p. 77, ll. 33-34). But Smith pre-
sents his "Arabic reading ... in contrast to the reading above" (p. 80, l.10). What a
"contrast"!: the letter f instead of p (l.8; cf. also below, p. 144) and "[y^{c}]ly" in-
stead of "y^{c}lly" (p. 78, l.9). That is all! Healey never mentions, explains or justi-
fies his erroneous spelling of "y^{c}lly" and his commentary (p. 79, l.10) gives only the

the reading "y^{c}ly"! Besides, the erroneous "d$^{\flat}$wmn" (l.8, instead of d$^{\flat}$ wmn) remains uncorrected by Smith!

b - The definitive ascription of the text to Arabic is held as certain by Healey, p.77, l.8: "the inscription is in fact in Arabic" because, in his opinion, "it is the fundamental grammatical structure of a text which determines its language, not incidentals like lexica and formulaic units taken from another tradition. In my view, the fundamental syntactical structure of JS 17, as shown above all by the syntax of l.1 and the optative perfects in lines 6 and 9, is not Aramaic" (p. 79, ll. 22-25). But, all that does not make them Arabic! The syntax of sn^{c}h (l.1) and the optative value of l^{c}n (ll. 6 and 9) have already been pointed out by the editors and J. Cantineau; such is the meaning of Healey's deceitful "in my view." Healey's position is quite logical because the expression "lexica and formulaic units taken from another tradition" (cf. below, pp. 129-130, O'Connor's position) proves that the choice of language was done earlier in favor of Arabic although JsaT 1 shows the author of JsaN 17 to be, not an Arab, but a Thamudaean (cf. already above, p. 109), and, subsequently, everything was evaluated according to that early gratuitous, preconceived falsehood.

Smith's swan song (p. 82, ll. 33-38) is the part of his paper which shows more clearly than any other how aprioristic, how preconceived his theory really is: it is built as a two-layer sandwich with an in-between filling. The 2 layers are respectively,

> "if we can accept that the language of JS 17 is Arabic" (l.33; upper layer) and
> "certainly JS 17 must be included among the mere handful of pre-Islamic Arabic inscriptions" (ll. 36-37; lower layer).

The upper layer explicits the result of Smith's painstaking efforts of his 91-line contribution (pp.80-82) which must evidently be the very best he could possibly expectorate. But they do not seem to convince even their suthor since all that he is able to say is "if we can accept" my thesis as if he wanted to say "if we can convince ourselves in spite of my inadequate argumentation" or "as we have argued" (cf. J.F. Healey in Aram, 2 [1990], p. 93).

The in-between filling looks very much like a summary of the possible benefits attractive enough to rousing the reader to the alluring advantages of the mirage. Then, as if to show the way to the reader, Smith becomes enthralled by his own game and falling - deliberately, no doubt - into his own snare, he holds up his head high and proclaims to his Manchester community and the whole world that his dream is too beautiful, too momentous to pass it up and, therefore, the mirage must actually be real; this is the lower layer!

2 - Healey.

The summary of Healey's position looks like a septimole, viz

a - a diacritical point above the letter d (p. 112);
b - the inital word of l.1 (p. 113);
c - the grammatical syntax of l.1 (p. 115);
d - the word br of l.1 (p. 116);
e - the optative perfect of ll. 6 and 9 (p. 118);
f - the case of šn³ of l.7 (p. 119); and
g - the next to the last word of l.9 (p. 119).

a - Healey categorically posits that the letter d of his reading (4 times only) "is consistently furnished with a point above it" (p. 78, l.14) to distinguish it from r. But, he immediately adds that

"in two other places there seem to be dots also on an /r/ and a /s/ (line 2), though since points are not consistently used with these letters it is possible that in these cases we have accidental flaws on the rock surface (which is in a bad condition) or scribal errors"(ll. 15-17).

The following remarks are most obviously in order.

(1) - The "/s/" must be corrected to š (cf. above, p. 107, no.8).

(2) - Since a dot would be - according to his mirage - above d, r and š, how could that dot be a diacritical sign to distinguish one letter from the other? And his "possible" is a mere deceitful latius hos invented for the sake of his cause.

(3) - Healey's note on the actual "bad condition" of the rock makes even more obviously dejected and loathsome his failure to study the 2 squeezes made at the beginning of this century, and especially the 2nd which Jaussen-Savignac themselves describe as "aussi parfait que possible" (cf. l.c., I, p. 481); cf. already above, pp. 108-109.

(4) - The sketchy facsimile of pl. 46B confirms the author's choice of the 2 words "point" = "dot," each of which means "a small roundmark" or "a very small mark (as a dot)." These 2 words, however, do not apply to the marks on d of ll.8 and 9, where it is a vertically elongated notch in l.8, but the lower half of a long mark curving to the left in l.9. Furthermore, contrary to Healey's so-called conviction, a vertically elongated mark is also found in 2 other places, viz to the right of the upper half of b of br (l.1), and above both m and r of mry (l.7). Moreover, a dot also appears in yet 3 more places, viz to the right of ʿ of ʾlmʿ (l.7), to the right of the lower extremity of q of yqbr (l.9) and also immediately to the right of the dot shown in Healey's facsimile above d of ʿbdmnwtw (l.3); this dot appears on the photo of Jaussen-Savignac's squeeze, but is a shady spot in Healey's photo of pl. 46A. Thus, 6 pockmarks are eliminated by Healey! The cases of a pockmark to the right of a letter must be taken into consideration: they indeed prove that the rock surface is pitted and, therefore, that those marks, even if real dots, can still be naturally made regardless of their position in relation

to the letters. To consider those marks, either all or some of them, as "scribal errors" because they do not fit a superficial empirical survey is a despicable, scandalous quibble in the reading, study and argumentation of ancient Arabic texts. Thus, the condition of JsaN 17 is such that a careful examen of the 2 very old squeezes made by Jaussen-Savignac is more than an usual necessity especially for anyone who rashly takes upon himself to find fault with those 2 editors. It takes an unusual dose of self-conceit and haughtiness to resolutely discard without even a word of explanation or even a mere mention of it the uniquely crucial testimony of the 2 squeezes made almost 90 years ago.

(5) - The presentation of Healey's exceptionally strange behavior on the last word of l.8, which was pointed out above (pp. 110-111), must now be brought to completion. A close look at the photos of both Healey's pl. 46A and Jaussen-Savignac's squeeze fails to show a dot above the letter. Is it the reason explaining Healey's peculiar behavior in the case? Obviously enough, this fact destroys the very 1st statement of his septimole and, consequently, the 2nd (cf. below, this page) also which is a mere speculation of the 1st. It thus stands to reason that Healey had no choice but to eliminate tooth and nail this most embarassing, desastrous d, the fifth of the inscription.

(6) - Healey's notes 7 and 8 (p. 83) refer to JsaN 181, 321 and Keiser 1 as other examples of the use of diacritical dots. As more than the lower half of p.84 still remains blank, it would have been easy for Healey to inform the reader that the documentation on the 1st 2 texts has not yet been confirmed by any photo (only copies were published), and that the diacritical point affects r (not d, as in JsaN 17). Finally, the reference to Keiser 1 does not, and cannot, prove the existence of diacritical dots in JsaN 17, even if taken for granted that it is not an original natural feature.

(7) - B. Gruendler's naïve gullibility as well as her superficial research are shown by her statement: "Healey's photograph and transcription [= facsimile] show diacritical dots on dhālet (and possibly rēs and sîn)"(cf. l.c., p. 11A).

b - The immediate effect of Healey's superficial work on his alleged man-made mark above d is that

"there is no diacritic over the first letter,... it cannot therefore be /d/ and the correct reading must be th" (p. 78, ll. 31-33) with reference to the haughty note 11 (p. 83), where the author peacocks about his claim:

"It may be noted that this conviction arises from examination of the rock-face rather than from examination of the photograph [cf. above, p.109], which gives the impression of an indentation in the rock above the /t/."

In other words, Healey demands the reader to endorse his "conviction" because of his "examination of the rock-surface," and, consequently, to forget about the testimony of the squeezes made ... almost 90 years ago! In fact, this self-preening turns out to be the

worst self-indictment against his so-called scientific method. His basis methodological error (cf. above, pp. 108-109) is evident and well planned: it is a clear case of cold-blooded premeditation, viz Healey voluntarily restricts himself to the PRESENT condition of the rock without paying any attention whatsoever to the 2 squeezes made almost 90 YEARS AGO, and/or even to the photo of the 1st of them published in 1909 by Jaussen-Savignac. This photo shows that the so-called "indentation," which may well have been dreamed of for the sake of the cause, did not exist at the beginning of this century. Healey was indeed blinded by his own partisanship. Had he been in a normal frame of mind, he might have been interested in the form of the letter t. It is, indeed, extremely easy to utter the oracular verdict of "the correct reading must be th" after spewing another pronunciamento according to which "doubt on this now seems to be eliminated" (p. 78, l.31) because of his "conviction" based on the absence of a point above the initial staff of the line. Unfortunately, his subjective "conviction" does not alter in the least the fact that the reading of t is clearly excluded by the 2 following observations which Healey should have gathered himself had he done his homework honestly, viz the systematical analysis of both the 5 d's and the 10 t's of the inscription. Cf. also below the last w of l.5 (p.120) and the space between JsaN 17 and JsaT 1 (cf. below, p. 137).

(1) The constant characteristic of the letter t is that the lower extremity of its right staff ends up in a well-developed inward curve regardless of the position, initial, medial or final (1, 4 and 4 times, respectively), of the letter. This pattern is somewhat altered in t of ʿbdmnwtw (l.3), where the lower half of the right staff makes a slight concave curve the lower extremity of which is definitely to the left of the projected upper extremity; cf. the photo of Jaussen-Savignac's squeeze. Healey's facsimile of that letter is very distorted on pl. 46B. In sharp opposition to this characteristic of t, the initial staff of JsaN 17/1 is straight and shows neither curve nor bend (as seen repeatedly in the plates of J. Euting, Nabatäische Inschriften aus Arabia, Berlin, 1885, and JsaN 1, 3, 5, etc). It is presumably Healey's inaccurate facsimile that B. Gruendler reproduces in her chart of p. 41, no. 16, right in the "Left connection" column.

(2) All the above-listed photos also show that the initial d is NEVER connected with the following n. This fact ALREADY is UNDENIABLY clear in Healey's photo as well as in the photos (now at my disposal) of Jaussen-Savignac's squeezes of the adjacent Thamudic text, JsaT 1, which, very fortunately, also show a few letters of the right edge of JsaN 17. For more details, cf. below, p. 137.

The 2 preceding palaeographical analyses are not even mentioned by Healey fuddled, as he is, by his inexistent, imaginary mirage, "which [as he haughtily pro-

claims] has until now gone unremarked" (p. 78, l.14). Summarily, Healey sees what he wants to see ... and nothing else. The beginning of JsaN 17/1 reads dnh qbrw, as already in the edition, which equals dn qbr² (e.g., But 2/1, 9/1, etc).

c - Healey's fundamental reason for ascribing JsaN 17 to Arabic is the pericope sn^{c}h k^{c}bw asyndetically apposed to dnh qbrw instead of the relative pronoun followed by the verb. Yet this reason exists only on the mind of those pan-classical Arabic fanatics. The syntax exhibited by JsaN 17/1 is well in the line of Thamudic texts, viz (1) the relative pronoun is very often omitted; cf., e.g., But 486, 549b, 697, Hu 2, etc (cf. my Miscellanées ..., V [1974], pp. 38, 39, 42 and 44, respectively), and also NaT 1 and 3 (cf. below, pp.142-143);

(2) the verb often introduces the phrase; e.g., JaT 20b, 24bis, 33b (cf. l.c., VII [1974], pp. 12, 52, 109 and pl.1, 13, 28, respectively), and the numerous phrases beginning with, e.g., dn Has perished (cf. also, below, NaT 2, 3 and 4, p.143), bn Has stopped, bt Has spent the night, and wdd Has loved; this phrasaeology is even followed by a Minaean in Ja 2284b , a graffito southwest of al-ʿUlâ (cf. l.c., V [1974], p. 63 and tracing in pl. 13); cf. also the ʾAlâb texts republished above, pp. 99-104.

A good parallel of JsaN 17/1 is Hu 104:

hym ʾtm ʾm wddh

Haym has engaged in sexual relations with ʾAwm: he was in love with him (cf. l.c., V [1974], p. 51; and Euting's facsimile in pl. 6A: But 108) =

Haym has engaged in sexual relations with ʾAwm whom he loved.

The possibility of translating the end of Hu 104 as "with the mother [ʾm, as in Hu 194] of Waddâduh" [wddh, as in Hu 394 and 395c] is not supported by the pharaseology of the ʾtm pericopes. For the sake of completeness, I have to mention E.A. Knauf making an exhibit of himself in the 2nd part (p. 155) of his 1989 Ismael (cf. below, p.147) where he refers to F.V. Winnett's paper published at the occasion of the 50th anniversary of his pamphlet A Study of the Lihyanite and Thamudic Inscriptions, Toronto, 1937, in JAOS,107 (1987), pp. 239-244, where, according to him, the author rejects my interpretations developed in JAOS, 88 (1968), pp. 290-294. But, the volume and year of Winnett's paper given by Knauf, viz his "106 (1986)" must be corrected to "107 (1987);" and Knauf is not honest enough to inform his reader that Winnett's 1987 paper was refuted to the letter the very same year, September 2, 1987, in my Miscellanées ..., XV (1987), pp. 124-191, the summary of which reads as follows:

> "The preceding pages contain an almost countless number of irregularities of all kind in Winnett's method which may be summarized as based solely on arguments for the sake of the cause regardless of self-contradiction, about-face, good common sense and any writing, not even his own."

Knauf's addendum of p. 155 refers to fn 483 (p. 87) where he lists 5 (out of 26) ʾtm texts, viz Hu 114, 128, 753, JsaT 409 and 576. As in ClAS 661 (cf. below, p. 147), Knauf does not even inform the reader of his choice and does not justify it. He neither explains nor justifies why ʾtm is replaced in some texts by ʾtm + n, and what might such a variant imply. He gives an incomprehensible translation of the 2 pericopes put into one, viz "O GN, stelle mich (bzw. PN bzw. mich, PN) wieder her!" (fn 483, l.4); "GN" and "PN" stand for "divine name" and "personal name" respectively. No explanation is given, viz to "restore" ("wiederherstellen") what: health, family, wealth, etc? He does neither inform the reader that he resuscitates A. van den Branden long-dead fallacy of ʾn as the 1st singular personal pronoun, while Naveh takes it for the plural form (cf. below, p. 145)! Summarily, Knauf is not honest enough to set aside his supercilious attitude and discuss what he dislikes while explaining and justifying his actions (cf. also below, p. 147), viz unwilling to justify or even to explain his writing, Knauf makes it impossible for anyone to deal appropriately with his work which looks very much like the freakish fanciness of a self-made deaf-mute. So be it: "Quod gratis affirmatur, gratis negatur."

d - As for the word br, the Aramaic form of bn son of, Healey claims that "it need not be regarded as a 'living' part of the language" (p. 79, l.37) or that it is "a fossil" (p. 80, l.4), and he sheepisly repeats W.W. Müller's oracle of "a virtual logogram which would be read in any language" (p. 79, l.39). A few remarks are in order.

(1) Müller's hight-resounding expression, "a virtual logogram," said about the 2-letter br is meaningless; it nonetheless remains unexplained!

(2) Both Healey and Müller fail to provide the reader with the following information, viz

(a) the proof of their contention, which remains gratuitous. Here again: "Quod gratis affirmatur, gratis negatur;"

(b) the origin of their remark. If it is still unknown where Healey picked up his idea, the origin of Müller's oracle does not seem to be in doubt (cf. below, p. 123);

(c) the purpose of their remark, viz what do they exactly intend to prove?;

(d) Müller's silence in the question of br in North-Arabic is symptomatic as he implicitely claimed to be the specialist in North-Arabic with both the prior consent and the post-factum defense of W. Fischer (cf. above, p. 108, no.11; my Miscellanées ..., XIV [1985], pp. 144-170 deal with Müller's conviction of being "the long-awaited messiah in NA" [= North-Arabic] by reviewing his statements on his Safaitic in W. Fischer's book, pp. 22-25; cf. above, p. 108, no. 11). In fact, br remains UNKNOWN in Liḫyanite, Thamudic, Safaitic

as well as in South-Arabic; and finally

(e) the justification of bnt in JsaT 1 instead of "brt" which would have matched brt of the Nabataean inscription, JsaN 17/2.

(3) To top it off and give another proof of his ineptitude, Healey claims that the presence of br in the an-Namârah text "makes it clear that it could be used within an Arabic text" (p. 79, ll.36-37). This remark is unequivocally a beautiful "lapalissade" stating that br is there, but the main question to be raised is why? Both Healey and Müller, who do not shrink from dabbling in parsing, are not even able to analyze correctly what they read. The an-Namârah funeral text amply proves the blunder of calling br "a fossil" or "a virtual logogram" because, on the contrary, it proves br to be so much alive that, in the 4th century A.D., it still was, along with the -w ending of proper names, part of a vigourous, seductive and quite irresistible distinction even in an Arab stronghold. When the famous Arab king ʾImruʾ-l-qays introduced br in his own titulature, he was officially renegating the title of his own father who had the usual bn, viz ʿAmr bin ʿAdî, and he further "Nabataeanized" the name of his own father ʿmr to ʿmrw; thus he made his titulature to be

ʾmrʾlqys bR ʿmrW contrasting with his own father's identity card

ʿmr bN ʿdy.

He also "Nabataeanized" 3 other tribe names, viz Nizâr as nzrW, Ma[d]ḥig as m[d]ḥgW (l. 2), and Maʿadd as mʿdW (l.3), after the mention of the Arabic dual ʾlʾsdyn and before the ordinary plural of bn in l.3. The situation of the an-Namârah inscription is misrepresented by J. Starcky (cf. "Pétra...," l.c., col. 932, ll.6-7), who claims that the text is, with the exception of the single word br son, entirely Arabic. Starcky's position is contradicted by the 4 proper names, viz that of the king's father and those of 3 Arabic tribes, all with the -w ending manifest what the only br could not do alone, viz the royal author had made up his mind to give his text the "Nabataeanizing" extra glamour of using br in his titulature and the 4 "Nabataeanized" names, including that of his own father, in flagrant opposition to his own father's titulature. It may be added here that the expression ʾlʾsdyn wnzrw wmlwkhm of an-Namârah (l.2) illustrates my suggestion on "Safaitic mlk, 'Lord' of the tribe" in Orientalia, 39 (1970), pp. 504-511. In the preceding lines I have taken the an-Namârah text as in R. Dussaud - F. Macler, Mission dans les régions désertiques de la Syrie Moyenne, Paris, 1903, pp. 314-315 and Dussaud's revised translation in La pénétration ..., p. 64. It is most unfortunate and incomprehensible that none of the latest studies of such an important text did not even bother to submit the squeeze made in 1901 by Dussaud-Macler (cf. R. Dussaud in Comptes Rendus de l'Académie des Inscriptions et Belles-Lettres, Paris, 1902, p. 259) to

a thorough examination and at long last produce an accurate tracing of the letters. Even A.F.L. Beeston's testimony is repeatedly reproduced by H.I. MacAdam (cf. al-Abhath 28 [1980], p.16) and by J.A. Bellamy (cf. JAOS, 105 [1985], pp. 32B-33A), viz "in part one is really forced to rely largely on his [= Dussaud's] readings." These 2 authors did apparently not realize - very strange, indeed! - Beeston's blunder of NOT EVEN MENTIONING the 1901 squeeze! Instead of futilely complaining about a situation he could have changed — and this remark is also valid for all other recent scholars — , the only thing Beeston should have done is to study the squeeze made BEFORE the stone started on its way to Le Louvre, a very long trip indeed during which it could have been damaged in many ways, and then to make a good tracing from a photo of the squeeze. But, of course, nobody could reasonably have expected such a rational, epigraphical approach from bookish, cerebral Beeston ... who has never been a true epigraphist anyway.

e - The 2nd and last pillar (cf. above, p. 112) of Healey's thesis is "the optative perfects in lines 6 and 9" (p. 79, l.25). But there is only one perfect, the twice repeated l^{c}n, which is related to Arabic la^{c}ana to reject, curse (cf. below, p.126). The editors of the text had already in April 1908 translated l^{c}n as "maudisse" and "qu'il maudisse" (cf. RB, 1908, p. 242). It is the imperfect yl^{c}n (e.g., JsaN 2/4; cf. also the expression "il^{c}an abouk ... Qu'Allah maudisse ton père" in A. Jaussen, Coutumes des Arabes au pays de Moab, Paris, 1948, p.5, fn 1) that clearly inspired the optative meaning for the perfect; cf. also M. Lidzbarski in "h$^{\flat}$lh l^{c}n 'Gott! verfluche ...'" of Hu 627 in Ephemeris für semitische Epigraphie, Giessen, II (1908), pp. 48 and 429A; for the text, cf. my Miscellanées ..., XV (1987), pp. 178-181. It is anachronistic to apply a peculiarity of the Arabic grammar to a Nabataean text while having no proof, or even without attempting to give a proof, that the classical Arabic particularity (cf. W. Wright, A Grammar of the Arabic Language, Beirut, 1974, II, pp. 2-3) was already used in pre-Islamic time in the northern part of Arabia. Furthermore, a verb preceding the subject is a well-known feature in Thamudic, Liḥyanite and Safaitic (cf. above, p. 115). Yet, that interpretation was applied to the Thamudic pericope wdkrt $^{\flat}$lt of Meek by P.V. Winnett in A Study ..., p. 6 (without acknowledging his copying of M.R. Savignac's interpretation of the formula in RB, 42 [1933], p. 412), where he corrects Jaussen-Savignac's rendering of JsaN 212. Winnett refers indeed to RB, 42, pp. "412-422" but without even mentioning the author's name, Savignac! Then, Savignac's understanding of the formula became a commonplace; cf., e.g., E. Littmann, Thamūd und Ṣafā, Leipzig, 1940, e.g., p. 83, no.139. In spite of this widely spread consensus - Quantitas non mutat speciem! - instead of being a wish, a prayer for the future, the formula simply is the acknowledgment of past blessings, a public act of gratitude: the divinity having in fact, as the text would say,

remembered or cursed So-and-So (cf. also below, NaT 7 in p.146); this interpretation is also mentiond by M.R. Savignac in RB, l.c., p. 413.

The Thamudic expression listed above, dkrt $^{\flat}$lt, is also found in JsaN 212/1 where Milik-Starcky apply the 1933 Savignac's interpretation, viz "Que $^{\flat}$Ilat se souvienne de" (cf. Winnett-Reed, l.c., p. 148, no. 42/1); parallelly dkyr of a text (very often at the beginning; e.g., in JsaN 215), is also understood as "Que soit commémoré" by Milik-Starcky (cf. Winnett-Reed, l.c., p. 149) who explain the form as "Perf. pass." (cf. l.c., p. 197B), and "Perf." applies to the 2 forms dkr (= "se souvenir") and dkyr. Cantineau explains dkyr as a "part. pass. sg. masc.: qu'on se souvienne de (gr μνησθῆ)" (cf. II, p. 82A) = one of the "véritables thèmes passifs ... dkyr ... Sens jussif attesté par les bilingues: μνησθῆ 'qu'on se souvienne'" (cf. I, p. 74), but "les participes actifs du thème simple tels que bryk 'béni', dkyr 'dont on se souvient'" (cf. I, p. 87, with reference to p. 74). The optative (not "jussive" because nobody gives such a command or order to anyone else and especially to a deity) meaning is clearly based on the bilinguals. But such an argumentation is unacceptable because it should be proven that those bilinguals have exactly the very same contents. This of course is impossible; cf., e.g., in the Greek-Nabataean bilingual RÉS 1092 = L 103, only the name of the deity Šay^{c} is found in both texts, and in the case now under study, JsaN 17 and JsaT I have each in common 2 names spelled differently: rqwš/rqš and ^{c}bdmnwtw/^{c}bdmnt. Thus, here again, my above-explained conclusion fully applies to texts, such as

dkrt $^{\flat}$lt ... (JsaN 42/1) $^{\flat}$Ilat has remembered ...; and

dkyr zydw ... (Jsa 215) Zaydû is remembered ... [here].

f - The presentation of the case of šn$^{\flat}$ (1.7) also shows how deep runs the apriorism of the 2 coauthors' pan-classical Arabic fanaticism. Healey finds "it. hard to see why the author [of a Nabataean text] has chosen an Aramaic vocable at this point" the reason being that "the verb ^{c}yr, an Arabism ... might have been expected;" consequently, "it may be suspected that there might be some other, 'Arabic' explanation of the word" (p. 79, last 3 lines). Healey is so infatuated with his preconceived Arabic mirage that he could not see the obvious, viz the use of the Aramaic verb šn$^{\flat}$ is manifestly at home in a Nabataean inscription and all the more so since the original meaning of that verb fits the context perfectly well. For Smith and the verb šn$^{\flat}$, cf. below, pp. 127-128. Finally, it is counterproductive as well as of very poor judgment to present the reader with the anatomical graphic of his own preconceived scheme. But the natural reaction of a saturated stomach is to disgorge.

g - Another important item of Healey's position is his new reading of the next to the last word of l.9 without, of course, mentioning (cf. above, p. 107) that

Chabot's reading of ʾʿly had been positively endorsed by Milik-Starcky (cf. l.c., p. 155) who affirm that their research was prepared "sur la base des photographies et quelques copies transmises par W.L. Reed and F.V. Winnett" (cf. l.c., p. 141). Healey, sheepishly followed by Smith, corrects ʾ to ỵ without any actual proof (cf. below, this page) and the 2 of them display at the occasion a pathetically inconsistent attitude which readily illustrates their mental instability:

Healey: "yʿlly" (p. 78, l.9) = "yʿly" (p. 79, l. 10) and

Smith: "[yʿ]ly" (p. 80, l. 19) = "yʿly" (p. 81, l.29);

in either case there is no acknowledgment, no explanation of the variant!

Healey affirms that "this new reading does, however, remain tentative" (p. 79, l. 11). This statement affects his 2nd reading listed above. But then, how can the reader understand that it is given in l.10 as absolutely certain since there is not even a question mark added to it? And what is the reader supposed to do with the 1st reading? Furthermore, his evaluation, whatever his reading may be, is all the more weakened and even doomed by his acknowledgement that the place of the 2 disputed letters "is very obscure" (p. 79, l.6). Here again, the basic methodological error of not consulting the 2 squeezes made almost 90 years ago (cf. already above, p. 109) destroys all Healey's effort and argument and even his credibility. Nonetheless, however, Healey dares to exclude the letter ʾ because of 2 valueless reasons.

(1) Healey's 1st argument is that "there is no trace on the rock-face of an ʿalif [sic!] of the size that is normal in this inscription" (p. 79, ll.9-10). How is it possible to understand that Healey is capable of coining a statement so clear, so precise and so final after looking at a "very obscure" spot of the rock? Furthermore, Healey's affirmation is contradicted by the small-sized letters of 11.8 and especially 5, each of which would certainly fit the space of 1.9. Moreover, since Healey speaks of "the size" "normal" of letters, the 2 letters in stippling in his sketch are without any parallel in the whole text on the double viewpoint of size and form. Finally, Healey has no right to talk of letter forms; e.g. the loop of the 3rd w of 1.9 is closed on the photos of both pl. 46A and Jaussen-Savignac's squeeze, but is wide open in his own funnies-like facsimile of pl. 46B. Cf. already above, the letters d and t (pp. 113-114) and below the space between JsaN 17 and JsaT 1 (cf. p. 137).

(2) Healey's 2nd argument against Chabot's reading is that it is "a perfect verb-form after an imperfect" (p. 79, ll. 8-9). But the presentation of his interpretation of ʾʿly as both necessary and unique is neither as ʾʿly may be an infinitive. Furthermore, the 2 coauthors' confusion of "yʿlly" = "yʿly" = "[yʿ]ly" = "yʿly" is further enhanced by Smith's commentary which ends on this typically Beeston-like insane statement: "one m

ust [sic!] assume perhaps wa-yuʿli [= the 4th active form] to be a grammatical error on the part of the scribe: yuʿli for the jussive yuʿli" (p. 82, ll.5-6). What kind of gibberish is this beside the insane abuse of an author accusing the engraver of an "error" because of the most insolent pretext possible: he does not understand the written word! what a senseless pity that Thamudaean Kaʿab dared to order his inscription before seeking the advice and approbation from the self-proclaimed omniscient G.R.Smith! What a shame!

From the double viewpoint of the quality of the information utilised and of the authors' professionalism, Chabot's reading based on an excellent squeeze made shortly after Jaussen-Savignac's expedition and fully approved by these 2 pioneers who had studied the original a few months earlier, cannot be compared with Healey's "tentative" guessing mostly based on the one hand on recent photos which are not even supported by the photo of the earliest squeeze and, on the other hand, on the wavering of the Healey-Smith tandem unable to agree on the initial transliteration of Healey's so-called discovery and to present acceptable or justifying statements.

Summarily, the editors of the text already spoke of "une inscription, encore nabatéenne dans l'ensemble mais tournant déjà fortement vers l'arabe" (cf. RB, 1908, p. 248) or a text which "marque une époque de transition, dans laquelle on voit le nabatéen en train de fusionner avec l'arabe, en adoptant des mots et des formes grammaticales très caractéristiques" (cf. Mission ..., l.c., p. 176). These 2 Jaussen-Savignac quotations belong to the picturesque style, which is also utilised by J. Cantineau, Le nabatéen, Paris, 1930, II, p. 39 (cf. below, p. 133, "O'Connor's position"). Healey's contribution is limited (1) to viewing the already well-known Arabic characteristics of the text in the light of a narrow-minded, abusive modern pan-classical Arabic partisanship; (2) to claiming the discovery of an alledged, partial system of diacritical signs; and (3) to suggesting a "tentative" (p. 79, l.11) reading of the next to the last word of l.9 as either "yʿlly" or "yʿly" (cf. above, p.120).

3 - G. Rex Smith.

Smith's contribution, commentary and notes, are conspicuously worthless, intended to be, as they really are, a mere rubber stamp, a slavish endorsement of Healey's preconceived theory and, therefore, doomed to be rejected out of hand. Their true value really lies in their showing how childish and irrational his so-called scientific method really is.

-a- In a futile effort to prove that JsaN 17 is written in Arabic, Smith presents his reader with his "Arabic with full taskil" [sic!!] (p.81, l.31). This initiative is deceptive since the Nabataean text was engraved by a Thamudaean, not by an Arab. It would still be erroneous even were the author of the text an Arab because it would imply the

impossibility of the vocalization being perfectly identical with that of classical Arabic. The same wile was also used by A.F.L. Beeston in his "cerebral juggling" (cf. my Carnegie Museum 1974-1975 Yemen Expedition, Pittsburgh, 1976, p. 95, and my Miscellanées VII [1974], p. 93 and my tracing in pl. 24) on the Liḥyanite JsaL 71 = JaL 118 in PSAS 3 (1973), pp. 69-72, an inscription which he, too, wants to transform into his Arabic invention, but not without making 12 (twelve) reading errors (cf. also below, p. 139).

-b- As a well-trained parrot illustrating his childish gullibility, Smith apishly speaks of "Sabaic where the word [= qbr] is invariably feminine" (p. 83, note 24), with reference to SDDS, p. 103, which states "n.f." My Miscellanées ..., XIV (1985), pp. 62-269, have already demonstrated abundantly the unreliability of that sham dictionary. In the present case, SDDS's information is very restricted, viz the masculine qbr in Sabaean dn/qbrn of CIH 618/2 and RÉS 4668/4, but feminine in Qatabanian Ja 343 A/3 and B/3.

-c- A beautiful example of Smith's devious argumenting method is that "sn^{c} [cf.l.1; to be corrected to sn^{c}] must in any case mean rather 'take care of' than 'make'" (p. 80, l.27) with reference to note 26 (p. 83) which reads as follows:

"JS 18, immediately adjacent, has the verb bnw, 'build' and provides us with the names of the 'builders'. For sn^{c}, cf. Lane, IV, 1732- 33 and Firuzabadi, Qamus, Cairo, 1952, III, 54." Here follow a few remarks.

(1) - The verb of JsaN 18/1, bnh (not "bnw," which is a plural form) and sn^{c} of JsaN 17 do not exclude each other. It is well-known in pre-Islamic that a person who claims to have built something (e.g., the mukarrib of Qatabân bny/mhfdn/mrd of Ja 3039/2, has built the tower Maris) did not perform the actual material building himself; he simply ordered the building and payed for it. Similarly Ka^{c}ab of JsaN 17 may justifiably claim to sn^{c} have made his mother's tomb; the masons of JsaN 18/1-2 merely executed Ka^{c}ab's order to build it and were remunerated for their work.

(2) - Smith also fails to consider that, Ka^{c}ab being Thamudaean, there is no indication in JsaN 18 that might even indirectly suggest that the masons and the carver were not Nabataeans. The difference of background between all those workers and Ka^{c}ab plainly justifies the lexicographical variations.

(3) - To justify his rendering of sn^{c} as "to take care of" Smith refers to 2 dictionaries. But, he carefully avoids informing his reader that "to take care of" is not the basic meaning of the root, but rather a derived meaning used when its direct complement is a horse or a girl (cf. E.W. Lane, An Arabic-English Lexicon, p. 1732, G.W. Freytag, Lexicon arabico-latinum, II, p. 525C, and J. Deboo, Jemenetisches Wörterbuch, Wiesbaden, 1989, p. 226). Moreover, Smith's meaning of "to take care of" is in the present case nonsensical. What would a man possibly do "to take care" of a tomb? This is not one of those well-aligned tombs on the neatly trimmed grasss of a modern cemetery; it

is "a tomb-niche cut in the rock-face," as Healey puts it (p. 77, l.20). Nobody in his right mind - with the apparent exception of G. Rex Smith - would even think of "to take care of" a rock or a few cemented ashlars blocking up a hole in the rocky wall. In other words, Smith anachronically and against good common sense applied a specific, derived meaning of classical Arabic to a pre-Islamic text of an entirely different context composed by a Thamudaean instead of reserving it for the only place where it belongs, viz in classical Arabic and in very special circumstances.

-d- Smith adds his own grain of salt to the question of br, viz br / brt "are always assumed to be North Semitic forms. But, bir is found in South Arabian Arabic" (p. 80,ll. 31-32) with reference in note 27 to Landberg, "Glossaire datinois"[sic!!],I, 1920, p. "142, sub brw." Smith should be congratulated for having finally discovered Landberg's reference more than 70 years after it was printed. But, he immediately puts his distorting touch by mentioning "sub brw" although Landberg's testimony is clear: "De cette racine br^{3} provient sans doute bir." By limiting his reference p. 142, Smith deliberately eliminates Landberg's explanation according to which "bir appartient au vieux fond des langues sémitiques" (p. 143). Landberg's preceding remark and its following commentary may safely be considered as the unavowed source of Müller's comment (cf. above, p.116). Moreover, Smith refers Sabaean brw exclusively (what else could he do?) to "Sabaic dictionary, 32," which refers to Raydān, 2 (1979), p. "99" (to be corrected to 98) where A.F.L. Beeston presents his imaginary, insane distinction between "biological child" and any other filiation (2nd generation or legal). This is Beeston at his best! Cf. my study in Carnegie... (not mentioned by Smith), pp. 94-95, which show that brw sometimes means "manager, guardian." Smith treads in Pirenne's footsteps (cf. below, p. 219).

-e- Smith claims that Nabataean "hrtt [sic] gives us the perfectly respectable Aramaic Harithah" (sic) (p. 80, l.35). H.P. Roschinski also spelled the name "Ḥāriṯat" (cf. Bonner Jahrbücher, 180 [1980], p. 162, where rqwš is also spelled "Raqāši;" this paper bears all the eclectic and pan-Arabic trademarks of the author's teacher, W. Caskel). How childish it is to ex cathedra declare Nabataean ḥrtt to be Arabic ḥrtt and to take it as another proof that the text is Arabic! Even were the name ḥrtt, only a pan-classical Arabic partisan could forget that the latter is well-known in Safaitic (cf., e.g., CIS 788, and ḥrtl in Thamudic HaLi 460, instead of ḥrt of the edition which eliminates the last letter) and in Sabaean (e.g., Ja 2724c), where the name has no relation with a Nabataean royal name. Even more importantly, Smith cannot prove - he does not even raise the question! - that ḥrtt of the text is not Thamudic ḥrtt, ḥrt being a root well-known in North-Arabic onomastics; cf. the well-known Safaitic name ḥrtt (e.g., Clark 3d), and ḥrt as a personal name in Thamudic (e.g., Hu 118) and Liḥyanite (JsaL

377), and Safaitic ḫrt, name of a person and of a place. Smith has no excuse, except his pan-classical Arabic partisanship, for choosing ḫrtt rather than the most immediately obvious ḫrtt. A last ditch objection would be to point out that the name ḫrtt remains unattested elsewhere in Thamudic. Such an objection would be completely irrelevant since the feminine name rqš (= Nabataean rqšw) still remains unattested elsewhere in North-Arabic and, more importantly, the root rqš is found nowhere else; thus, the name itself is a perfect hapax legomenon in the whole North-Arabic.

-f- The feminine name rqšt (1.2) launched Smith in a wild frenzy. He 1st claims that "there is certainly the Arabic feminine name Raqas (Raqasi, of the pattern fa'ali)" (p. 80, ll. 37-38; each "s" must be corrected to š (cf. above, p. 107, no. 8); cf. above, p. 123: Roschinski also spells "Raqāši". Of course, no proof is given for such an aberrant, sweeping juggling which is solely based on the author's pan-classical Arabic partisanship. The qtwl derivation is known in Thamudic. Then comes the author's recantation because of the "orthography" (p. 80, l.38) of the name ... as if he did not see it before. And since he saw it before - it would in fact have been impossible for him not to see it - how can he justify the 1st erroneous, useless juggling? Is it not a perfect example of the worst newspaporial mentality to print something even when it is known to be nonsensical? Follows then a 3rd ridiculous expectoration: "the word is not to be compared at all with the first one of l. 3 below" (p. 80, ll.38-39), viz with ʿbdmnwtw, and in fn 32 (p. 84) he refers to O'Connor's vocalization of the name as "Raqāsh" (to be corrected to "Raqāsh")." Of course, Smith fails to say that O'Connor copies in his own way F.V. Winnett, A Study ..., p. 38: "Raḳāsh." When the reader peruses Smith's commentary on ʿbdmnwtw (p. 81, ll.5-6), he understands that his previous remark in fact rehashes what has been known for a very long time (cf. also his discovery of Landberg's dictionary above, p. 123), that the 1st w of ʿbdmnwtw is a radical letter and -t a tâ' marbûṭat (as in ṣalwat = ṣâlat of the radical ṣlw, and zakwat = zakât of the radical zkw, both examples given by Smith) while w of rqš is a lengthening letter. Therefore, Smith's remark is a "lapalissade" which enables him to triumphantly proclaim his discovery which has already been given by Jaussen-Savignac, l.c., p. 172: "Raquš" and informs the whole world that "we must accept that the waw here gives us Raqus or Raqqus" (p. 80, l.39) with reference to note 33 (p. 84), another fruitless wandering among the Arabic formations "fa'ul ... fa'il ... fa''ul," uselessly based exclusively, here again, on his pan-classical Arabic partisanship. How ridiculous and boring it is that all those partisans selfishly waste the time of their reader and abuse his patience by imposing upon him the reading of their own uselessly extravagant, anachronistic rigmaroles! When are they going to wake up? But, why should they since they are openly endorsed by Academies and universities? For other examples of the same attitude, cf. above, pp.50 and 75.

-f- Smith's elucubrations (p. 81, ll.8-13) spurred by the verb hlkt (feminine of hlk, l.4) are equally senseless. According to him, al-Ḥigr, the place where the lady rqš died, "was not here [sic!] usual place of abode" (ll. 11-12) because the mention of "the place of death" (l.8) was "surely rare" (l.8). Which hoepoe or jinn did whisper to him that among the many dn Thamudic texts memorializing the passing of an individual (cf. already, p.115), the overwhelming majority of them were commemorative only? At any rate, Smith can find below (p.143) another Thamudic text, NaT 2, mentioning the exact place of death of another Thamoudaean. Furthermore, Smith's opinion is contradicted by the 2 following facts that were distorted by the narrow-mindedness of his partisanship.

(1) — He claims that the author of RÉS 3608 "was presumably a Minaean merchant visiting Dedan where he happened to die" (p. 81, ll.10-11). Here again, how does he know? How can he prove his "presumably"? What is the name of the hoepoe or jinn who whispered to his ear that that person was NOT a resident of Dedan, viz one of those officials, merchants, others or relatives, all members of that northern Minaean community? Had Smith been a true scholar - but how could he be, hypnothized, as he is, by his fanatic pan-classical Arabic partisanship the spell of which he apparently never attempted to shake off? - he might have thought of checking the corpus of the inscriptions from the country of al-ʿUlâ. Had he done it, he would have seen that the family/clan yfʿn Yafaʿân to which the author of RÉS 3608 belongs, is attested much more frequently than any other name at al-ʿUlâ, viz no less than 19 (nineteen) texts (cf. my Miscellanées ..., VII [1974], p. 181A), with the next in importance ʿmrtʿ with 12 texts, dtʾ with 6 texts and, at the bottom of the list 39 family/clan names mentioned only once each, and even both Ja 2340c and RÉS 3608 contain the very same identification card, ʾwsʾl dyfʿn. Confronted by such an information, any newsmonger would undoutedly claim that the 2 texts refer to the very same person.

(2) — Smith also claims that "this is 'a grave', therefore, not 'the grave' at home where she would have expected to have been buried" (p. 81, pp. 12-13). The reader would like to know what is the real difference between this statement and that which is given above, in (1). Here again, how does Smith know? His pan-classical Arabic partisanship prevented him from checking the rules of the article in Liḥyanite (cf. my Miscellanées ..., VII [1974], p. 101). Moreover, his distinction between "'a grave'" and "'the grave' at home" is most ridiculous. How does Smith know that lady rqš actually had already a tomb prepared or planned for her somewhere else? Did the lady talk to him in a crystal bowl or did she send a hoepoe to him with a special message? It is inconceivable that it would not be her grave because the tomb containing the mortal remains of a lady cannot be called "'a tomb'" by the deceased woman's son.

By the way, Smith's idea of the lady rqš being a visitor in Dedan comes from M. Clermont-Ganneau's remarks brought on by the pericope hšy wldh (ll. 8-9) and acknowledged by Jaussen-Savignac (cf. l.c., pp. 175-176, fn 5) who write this:

> "M. Cl.-Ga. suggère l'idée que Raquš serait peut-être morte fortuitement à Ḥégra; de plus, la tombe elle-même produit l'impression de quelque chose d'un peu provisoire. Ka'abu, n'aurait-il pas eu, par hasard, l'arrière-pensée de faire reposer plus tard sa mere dans un monument plus digne d'elle, élevé à Ḥégrâ ou ailleurs?

The reader is not really surprised that Smith does not acknowledge the source of his suggestion because he knows how easily it is to believe that nobody will check the past litterature and find out. The remark on the tomb producing the impression of something rather temporary is so vague and remains without even a single detail to substantiate or describe it; therefore, what else can be done except to take it as a hazy, empty rambling, the crazy idea of an imagination lost in the air and in search of a solid ground to set its feet? The simple appearance of a mere hole in the rocky side might have triggered such a reflexion of pauperism or of temporariness. But, who can doubt the probability of an elaborately decorated placard placed in front of, and attached to, the ashlars blocking the hole? Of course, such an adornment would have been the 1st item to be vandalized. Moreover, the hypothesis of a plan of later giving the lady another, more dignified (?) resting-place is contradicted by the material presence of the triptych composed of JsaN 17 + JsaN 18 + JsaT 1. How is it possible that it never occurred to any of those imaginative pan-Arabic classical fanatics to ask himself the following question: why would Ka'ab have gone through the great expense of no less than 3 inscriptions totaling 13 lines composed of 190 letters if he was contemplating on transporting his mother's coffin somewhere else? And what would he have done with those inscriptions after the transfer, destroy them by smashing or excision? Furthermore, the pericope hšy d(y) wldh does not justify any of the conjectures presented in the above quotation because it implies that any descendant of the lady had the basic right to open the tomb according, no doubt, to the family customs and decisions, e.g., to add the coffin of one of her children, as Jaussen-Savignac already pointed out: "Dans certains cas un membre de la famille aurait donc pu ouvrir une tombe" (cf. l.c.). This remark clarifies what was implicit in their translation of ll. 8-9 as "à l'exception de sa postérité" (cf. l.c., p. 173). Smith, however, translates wldh as "his offspring" (p. 82, l.10) = "waladahu/waladihi" (l.4), although this man, k'bw, is lastly referred to in l.3, and the closest person to be mentioned is lady rqwš in hy hlkt (ll. 3-4). Furthermore, the attribution of -h of wldh to k'bw would imply the improbable situation of k'bw acknowledging in his children a right which he would not have had himself. For dy wldh, cf. dw wldh of an-Namârah/4.

Summarily, the actual presence of the 3 inscriptions at their original places and without any attempt at erasing them, proves apodictically that Ka'ab never had any idea of transferring his mother's coffin somewhere else.

-b- Then, Smith posits that "here [= l.6] for the first time we have in the first three words of the line pure Aramaic" (p. 81, l.15): that they are printed in italics in p. 80, l. 16, and note 22 (p. 83) makes very sure that the reader does not overlook Smith's pompous, but empty gift to Nabataean. And the whole process, viz italics and reference to note 22, is repeated for the 1st 2 words of l.7. Smith's magnanimity to Nabataean is touching, but merely phony because he gives to Nabataean nothing that was not already agreed upon. And, the use of italics is deceptive because the whole lines 6 and 7 are printed in italic letters including, e.g., the verb l'n which is claimed to be Arabic, and this is a gross error; cf. already above, pp. 121-122.

-i- The perfect l'n in both ll. 6 and 9 is understood as having an optative meaning. No justification has ever been given; cf. already above, pp. 118-119. There is no reason for rejecting the obvious, normal meaning of the perfect tense, viz l'n mry 'lm' means the lord of the universe has cursed.

-j- The object of the curse is fourfold: each l'n introduces a set of 2 activities. The 4 operations, which are listed according to a crescendo of gravity, must not be coupled in 2 groups each of which being a single object of the curse. Each of those 4 deeds is in itself a serious offense and deserves to be the object of a curse.

The 1st group (ll.6-9) deals with 2 deeds involving the frame or the outside of the grave, viz yšn' 'lqbrw d' (ll. 7-8) and yfthh (l.8), 2 actions against the appearance of the tomb and its integrity.

The 1st action is defined by šn' = to change the appearance of the monument, thus to deface (cf. above, p.119). Characteristically Smith is unable to understand that kind of damage; and the reason remains the same, his pan-classical Arabic partisanship. Of course, Smith had to have recourse (what else could he do?) to Arabic šana/i'a to hate (an idea already suggested by Healey who was following the Syriac trail) and, according to the A.F.L. Beeston-like demented argumenting method involving the twisting, manipulating and juggling of ideas, he does not shrink from shifting the original meaning of "to hate" to that of "'spoil' or even 'desecrate'" (p. 81, ll. 22-23), as if those 3 verbs were synonyms! But, Beeston inured his friends and readers alike to that kind of acrobatic extravaganza, and the slavish admirer, Rex Smith, here follows his idol.

The 2nd action is much more malicious and devastating in nature because it involves the opening of the tomb, fth to break in, which necessarily implies the intention of putting an end to the secrecy of the tomb. Incidentally, note the parallelism of

dnh qbrw (l. 1) and
ʾlqbrw dʾ (ll. 7-8).

Smith's commentary emphasizes the masculine dʾ (p. 81, l.25), but the reader cannot forget his acrobatic explanation of Healey's "th qbrw" of l.1, which includes the statement according to which "the author chooses to make it feminine" (p. 80, ll.24-25) in the same text! What a joke! The pan-classical Arabic partisans, especially Beeston, the leader of them all, are to be admired for the apparently boundless resourcefulness with which they find explanations even to ... erroneous readings!

The 2nd group of curses (l.9) completes the 1st series by mentioning the worst mistreatments of the tomb possible: more than the appearance or the integrity of the tomb are violated, the contents of the tomb itself would be altered. There is a logical progression in the line of the 2 verbs of the 1st group, šnʾ and fth (ll. 7-8), viz after the defacing and the break-in of the tomb, there can only be question of the interment (qbr) of another body and the disinterment (ʾʿly) of lady rqwš. There cannot be any question of the removal (ʾʿly) of any body other than lady rqwš's because such a possibility did not concern kʿbw. The wording of 1.9 implies the removal of lady rqwš's body after the interment of another corpse. Such a contingency is possible only if the tomb is wide enough to hold 2 bodies at the same time. As for the reading of the 2nd half of 1.9, it should be emphasized that the obvious parallelism of ll. 6-9 and 9 ascertains that the subject of lʿn (ll. 6 and 9) remains the same, viz mry ʿlmʾ the lord of the universe. The situation is so obvious that the editors themselves (cf. RB, 1908, pp. 244-245, and Mission ..., I, pp. 175-176) did not even comment on it. It is, therefore, impossible to understand Smith's folly of taking lʿn of 1.9 as a passive form, an hypothesis whih remains without a single word of justification or explanation; cf. his "luʿina May ... be cursed' (p. 82, ll. 4 and 11). Parallelly to the 1st series, the 2nd group of curses also comprises 2 verbs, viz qbr and ʿly, according to Chabot's reading "mn yqbr wʾʿly mnh" published by Jaussen-Savignac, l.c., p. 481. Cf. already Smith: "perhaps 'disinter'" (p.81,l.30).

4 - The text of Jsa 17.

The aforementioned critics of both Healey's and Smith's opinions may be summarized as follows in the reading of the text.

1 dnh qbrw ṣnʿh kʿbw -
6 - - - wlʿn
7 mry ʿlmʾ mn yšnʾ ʾlqbrw
8 dʾ wmn yfthḥ ḫšy d(y)
9 wldh wlʿn mn yqbr wʾʿly mnh

1 This [is] the tomb which has made Kaʿab, -
6 - - - And has cursed
7 the lord of the universe anybody who would deface this very
8 tomb or anybody who would break into it except her
9 descent.And He has [also] cursed anyone who would inter or disinter from it.

II - O'Connor.

Being "also sceptical of the idea that the text is actually a mixture of Aramaic and Arabic" (p. 79, ll. 25-26), as if he had forgotten what he himself calls "the distinctively Aramaic elements in the text" (p. 79, l.21; viz only 5 lines above the preceding quotation!), Healey refers to his note19 (p. 83) where he makes a point in singling out "M. O'Connor, JNES 45 (1986) 213ff., rightly criticising [sic] the rather romantic view of this presented by J.Cantineau, Le nabateen [sic] ..."

Such a selection cannot but incite the reader to pay special attention to the literary production of this new detractor of one of those pioneers whose hard clearing work is unfairly weighted on contemporary scales. It would indeed be cruelly unfair to entomb in some hardly read footnote the feat of such unselfish a Knight rushing to the rescue of allegedly misunderstood and maltreated Dame Arabic. But, cf. the next page for O'Connor's incomprehensible example of blind condescendence to Starcky. Many remarks of the preceding pages already concern O'Connor; there thus only remain a few more to be added here.

1 - O'Connor position. - After acclaiming J. Cantineau as one of "the most theoretically sophisticated of all Semitists" (p. 214,l.14) - the usual sedative shot intended to put anyone out of guard - O'Connor immediately turned around and fired his missile, viz "Cantineau's statement of the relationship between Nabatean and the North Arabic dialects spoken by its users is erroneous" (cf. l.c., ll. 16-17).

To prove his point he refers to J. Cantineau, l.c., "vol. 1, p. x," where, according to his interpretation, "there is a streak of romanticism" (cf. l.c., ll. 27-28), and even he quotes (1.29) M. Cohen's disagreement as "Doctrine erronée" published by Cantineau himself (cf. l.c., p. 220).

a - This statement directly refers to the 2nd element of what he calls "Cantineau's vacillating treatment" (p. 222, ll.3-4); and the adjective "vacillating" is further repeated in the same page, fn. 46, l.4; and in l.3, O'Connor refers to J. Starcky's paper on "Palmyre," "cols. 1097-98" which, surprisingly enough, deal with the "dieux babyloniens"! In fact, there is here the confusion between Cantineau's commentary and his suggestion on the Nabataean-Arabic relationship; and there is no "vacillating position"

between the 2 sections since the 2 deal with different viewpoints.

b - Very regrettably O'Connor also failed to inform his reader of Cantineau's answer to Cohen's critic, viz "Mais je ne puis que constater les faits que nous fournissent les textes" (cf. l.c., p. 220). I cannot find a single good reason for O'Connor hiding Cantineau's reply from the reader. The simplicity and honesty of this answer characterize Cantineau's matter-of-fact style where "romanticism" or even, as O'Connor puts it, "a streak of romanticism" has no place (cf. also below, p. 141). Cantineau's phraseology belongs to the picturesque style, which O'Connor did not understand (cf. below, p. 133). In the same line of thought, O'Connor refers (p. 214, fn 8, l.2, but in l.4, "col. 925" must be corrected to col. 924) to J. Starcky's "more cautious" assessment of the situation, which speaks of "un certain nombre d'arabismes, dont l'origine n'est d'ailleurs pas homogène" (cf. "Pétra ...," col. 924). Where did Cantineau state that they were homogenous? O'Connor repeatedly refers to "J. Starcky's masterful summary, 'Pétra ...'" (p. 213, fn 1, ll. 37-38) in pp. 214, fn 8, ll. 4 and 7; 215, fn 11, l.2, 12, l.1, 14, l.2, etc), and all those references are to col. 924-925. Yet, he NEVER mentions the double fact that is practically the main goal of his own paper: Starcky claims that Jaussen 17 is "presque entièrement arabe" (cols. 925, ll.3-5, and 932, ll. 30-34), and that "il n'y a pas à en tenir compte pour l'analyse de la langue nabatéenne" (col. 925, ll. 5-6; cf. already above, p. 108). Both statements are manifestly exaggerated and unacceptable. Who is going to understand how the righteous Knight O'Connor could caracterize Starcky's position as "more cautious" when "un certain nombre d'arabismes" becomes the equivalent of "presque entièrement arabe"? This is much worse than "a streak of romanticism;" it is a plain exaggeration leading to a gross error of evaluation; cf. also above, p. 117, Starcky's manifestly exaggerated representation of the an-Namârah inscription. The reader cannot but wonder why O'Connor so plainly misunderstood both Cantineau and Starcky. The self-evident explanation that presents itself is both a linguistical reason, viz O'Connor did not really understand what he read, and a complete social protection, viz no author would answer him. It may be added that O'Connor's self-justification based only on M. Cohen's testimony would amount to mere sheepishness. Finally, O'Connor would have been well advised not only to inform his reader for the sake of complete objectivity and of his own minuteness (cf. below, p. 132), but also to take it as a personal lesson that René Dussaud chose not to take side in the Cantineau-Cohen misunderstanding (cf. La pénétration ..., p. 63).

c - Then O'Connor goes on defining how to proceed with his "evaluation" of the language.

(1) "The first step must be to insist that the personal and other names in

Nabatean do not bear on its evaluation as a language" (p. 214, ll. 29-30 - p. 215, l. 1). Cf. above, p. 117 on the an-Namârah text. O'Connor confuses 2 distinct realities, viz the origin of a name and its actual transliteration; e.g., names with the -w ending point to Nabataean in the same manner that names with the -m (mimation) ending refer to South-Arabian. It has always been a "lapalissade" that the letter forms of one language may be used by persons speaking another language. It cannot be denied, however, that the use of a certain type of letters or of specific nominal forms is a positive, intentional sign given by the authors to show the direction of their interests and preferences, and no one has the right to ignore them. To simply discard them right off amounts to a betrayal of the author because they are a part of his text as well as any other section. It further deprives the writer of objective clues as to the personality of the author and therefore of his work, viz to ignore them is to take voluntary risks to misjudge both the value and the interpretation of the text.

(20) "The second step must be to evaluate Cantineau's list of borrowed words and forms" (p. 215, ll. 1-2). After his tedious study (pp. 216-219), O'Connor proudly proclaims that "We have thus reduced Cantineau's list from twenty-nine to eigthteen Arabic loans by reconsidering the lexica data" (p. 219, ll. 3-4) OR "the Arabic loan vocabulary is smaller and more specialized than he [= Cantineau] was led to imagine" (p. 229, ll. 5-6). Such a positive result should normally generate ample recognition and generous applause. In fact, however, nothing of the kind is forthcoming because, somewhere else and, to make it very inconspicuous, at the end of a fn (p. 218, fn 27, ll. 13-16), O'Connor acknowledges that

> "In a number of cases in the list of rejected loans, the determinations are based on evidence that has come to light since Cantineau wrote; in some cases I may simply have missed his point" (the underlining is mine).

This is a rather unusual avowal of culpability, and there are no cross-references! It would also have been so easy for O'Connor to itemize the information hidden under his expressions "in a number of cases" and "in some cases." Obviously enough, it is much safer as well as much less incriminating to remain vague. Strange logic in a writer who presents himself to his reader in his very 1st fn (cf. immediately below) as "Mr. Minutiae" with his pockets full of the best detailed information possible! (cf. however above, p. 130 which points out that O'Connor failed to report an important information of the Cantineau-Cohen case). Last, but not least, the reader cannot understand why O'Connor did not ponder his above veiled apology, "I may simply have missed his point," BEFORE launching his missile (cf. above, p. 129) because he misunderstood Cantineau's stylistic license. This remark seems also to apply to his misunderstanding of Starcky's expressions (cf. above, p. 130).

2.- The end of O'Connor's fn 1 (p. 213B; the longest of the paper with its 71 lines sh
ed at projecting himself as the model of a very minutiose author!)
reveals the true image of the writer and gives the reader a general idea of what he ma
expect to find in the paper.

In the next to the last paragraph he gives the abbreviations of no less than 14 (four-
teen) languages "used throughout" (ll. 61-62). Yet, the 2nd in importance, Thamudic, is
WANTING!!! This is quite incomprehensible since he himself speaks of "a bilingual Naba-
tean-Thamudic text," transliterates and translates the Thamudic text (p. 221, ll. 17 and
20); but almost 8 pages separate the 2 positions! Furthermore, the last paragraph of
the same fn 1 gives the names of 6 (six) consultants. Of course, such an information is
always aimed at attracting sympathy by flattering the lucky chosen. But, here again, no-
ne of them is specialist in Thamudic and North-Arabic; and the balance of the paper show
that O'Connor is not a specialist in North-Arabic either. Therefore, O'Connor will speak
his mind, deliver sentence, and pass judgment on matters he,himself, and/or any of his
consulted friends cannot objectively claim to know!!!

O'Connor's ignorance of North-Arabic is proven by his statement (p. 215B, fn 14,
ll.10-14): "I shall assume the relative homogeneity of North Arabic, represented by both
Classical Arabic and the pre-Islamic dialects (Lihyanite [sic], Thamudic, Sa-
faitic), though there are differences among them."

The expression "the relative homogeneity" sounds highly technical and very learned, but
what is it supposed to mean, if anything? How can a responsible person write such a
sweeping statement when he cannot claim to know any of the pre-Islamic dialects? In fact,
he is so well informed on North-Arabic that he still repeats in 1986 a misunderstanding
of J. Cantineau in Le nabatéen, I (1932), p. 172: "ʾḫr as Lihyanite [sic]" (same fn 14,
l.8). Lihyanite "ʾḫr" does not exist; what exists is ʾḫrt, 4th form of ḫrt to guide, as
already demonstrated by A. van den Branden in al-Machriq, 54 (1960), p. 101. F.V. Win-
nett attributed to himself A. van den Branden's idea (without, of course, mentioning its
origin) in Winnett-Reed, l.c., pp. 126 and 193A. It should be emphasized that O'Connor
refers to the same work of Winnett-Reed in the same fn 14, l.18. The reader may thus con-
clude that O'Connor utilizes the information he likes and discards what he dislikes.
O'Connor repeats his performance in the case of "ʾṣdq ... as probably so" (cf. l.c., l.9),
although ʾṣdq is a name, not a noun.

3.- O'Connor also speak of "The Raqash Epitaph ... a bilingual Nabatean-Thamudic text"
(cf. p. 221, l.17). Healey has repeated the very same error (cf.
above, pp. 105-106). Cantineau could not after all have described too badly his so-
called "romantic view" since O'Connor deigns to still call JsaN 17 ... Nabataean. A
basic difference between O'Connor and Jaussen-Savignac, however, is that the latter (cf.

Inv., p. 173: "Raqūš;" cf. also Cantineau, l.c., II, p. 38: "Raqōš") did not surrender
to Arabic as O'Connor does although they were fully aware of the Arabic name "Raqāš"(p.
174); but O'Connor is apparently unaware that the vocalization "ʾr-rqyš" (in the name
ʾabū r-rqyš) is also found in F. Wüstenfeld, Jacut's geographisches Wörterbuch, Leipzig,
VI (1870), p. 431. In his very next page (p. 222), however, O'Connor's final oracular
pronunciamento is that JsaN 17 "must be excluded from the main body of Nabatean texts
and given a separate accounting" (ll.4-5); this is the same position as Starcky (cf.
above, p. 130) under a different cowl. What a pity that O'Connor did not give a name or
a siglum to his new creation! Maybe one could think of a siglum, such as "O'Connor In-
vention 1," or better yet, an accordion-like name "à la Robin," e.g., "O'Connor, Kʿab
Arabic Independent Nabataean 1."

O'Connor's reason of his verdict in fact remains a nonsense, viz "there is no trajectory
of Nabatean 'turning into' Arabic (p. 222, ll. 7-8). It is absurd to even imagine the
possiblity of a "trajectory" of growth parallelly to a decrease in living things in per-
petual motion. But his wording proves that he misunderstood Cantineau's picturesque sty-
le as realistic (cf. above, pp. 129 and 141). And, O'Connor, who revels in burying his
reader under his endless references in text and footnotes (cf. above, p. 132) fails to
mention where he copied his expression put between quotation marks, viz "'turning into'
Arabic." Although used in a phrase dealing with Cantineau, the expression yet comes from
a paper which I failed to find mentioned in O'Connor's prose, viz RB, 1908, p. 248, l.
32, where Jaussen-Savignac utilize the expression "tournant déjà fortement vers l'arabe"
(cf. already above, p. 121). Furthermore, O'Connor also fails to inform his reader that
both W.W. Müller, another pan-classical Arabic fanatic, and J. Starcky had already sug-
gested respectively in 1982 and 1986 (cf. above, pp. 108 and 130), a solution as drama-
tic as his own. Thus, when all is said and done, O'Connor's paper actually breaks into a
widely open door and, as such, is as useless as it is harmful because of all his biases,
political reasons and lack of North-Arabic information, especially Thamudic.

4.- At the end of his so-called study of JsaN 17, O'Connor distributes the items of his
inventory under 2 fuglemen, viz Nabataean and Arabic (p. 227). But his
sorting is vitiated by 1 basic error and 2 obvious biases.

a - The basic error is the author's pan-classical Arabic partisanship by which
his Arabic, a distended classical Arabic, has, like an insatiable snake, ingurgitated the
North-Arabic slugs. Consequently, there is never - and cannot be - any question of Thamu-
dic (cf. above, p. 131), viz the actual contribution of Thamudic is voluntarily ignored
and committed to the oblivion of a reptile stomach.

b - The sorting is vitiated from the very beginning by a well-calculated exag-

geration manifestly intended to deceive the reader; the initial statement reads as follows: "the names are largely Arabic; Aretas (ḥrtt) is notably Nabatean in background" (p. 227, l.5). In fact, there are only SIX names mentioned in the text, the Nabataean tmwz; the "Nabataeanized" Arabic ʾlḥgrw, and the 4 personal names kʿbw, ḥrtt, rqwš and ʿbdmnwtw. Thus, according to O'Connor, to be "largely Arabic" at least 4 out of the 6 names should be Arabic, viz ʾlḥgrw and 3 others which remain unspecified. Regardless of which might be the selected ones, tmwz is Nabataean, and the last 4 personal names need not be Arabic: kʿb is attested in Thamudic But 171 (cf. also Safaitic, e.g., CIS 5333); ḥrtt and rqwš, cf. already above, pp. 123-124 and 123, respectively; and ʿbdmnwtw also is found in Thamudic Hu 362.

c - The 2nd bias is an insult to logic: it amounts to the elimination of what manifestly contradicts his thesis prior to his actual itemization of the material, viz "if we exclude the nine naming words ... and the six words of the date ..., we are left with the following schematic text" (p. 227, ll. 7-9) upon which he bases his following inventory (ll. 14-19): out of 19 words, 10 are found in both Aramaic and Arabic;
7 in Arabic only; and
2 in Aramaic only.

The following remarks are in order.

(1) O'Connor has no right to exclude those 1st 15 words. They rightfully are an important part of the whole text, and their exclusion is a mere trick intended to lead the reader to forget about them.

(2) It is rather astounding to see O'Connor incapable of properly inventorying a very limited material.

(a) He counted the indefinite pronoun mn (ll. 7,8 and 9) twice; thus, the numbers "19" and "10" become 18 and 9, respectively; and

(b) he lists "the nine naming words (six in [ll.1-3], ʾlḥgrw in [l.4], mry ʿlmʾ in [l.7]" (l.8). Where is the 9th name? It is the month name tmwz (l. 6). In his simplemindedness O'Connor took the 2 sections of the single name mry ʿlmʾ for 2 different names!

(3) The purpose of deceiving the reader is manifest since O'Connor counting spree ends right there; those are the only figures he wants the reader to remember.

(4) To stop the counting process right here is all the more nonsensical that the 15 words so brutally set aside do not even belong to the same category of words; in fact they belong to 3 different categories.

(5) O'Connor's tally must be revised, and the following notes deal with 3 words still in need of further explanation.

a - O'Connor keeps repeating, although erroneously, that the verb lʿn is Arabic (cf. already Atlal, X (1406/1986), p. 112, l. 8). How would he know that lʿn already exists in Thamudic? Even had he found it in some publication, it would have made no difference whatsoever because of his pitiless buldozing of North-Arabic into the stomach of his pan-classical Arabic snake (cf. above, p. 133). What can be expected from such an erratic frame of mind is an awry stand comparable with an unnamable ragout or stew taken by blind O'Connor to be some broth because he does not know any better. For lʿn, cf. also above, p. 126.

b - hlk: O'Connor lists hlk along the "seven words" ("ʾḫr is the 1st of them; cf. above, p. 132) whose "relevant sense is not attested in Aramaic," with as 1st reference "BHeb, etc., 'to go', anglice 'to pass on'" (p.217, l.14). This information is manifestly incomplete and biased because in fn 25 (p. 218A, ll.5-6) he admits that Hebrew hlk sometimes means "to die." Whether or not used as "a [sic] euphemistic substitute" (cf. ll.11-12) is pointless since the meaning is clear and certain. Also, in ll. 14-18 of the same fn 14, O'Connor mentions "a Thamudic text" - he is even incapable of listing the accurate siglum, WiTa 3 ((where is his minuteness?; cf. above, p. 132, no.2)) - where he refers to my review of Winnett-Reed's 1970 booklet in Orientalia, 92 (1972), p. 486. The discussion of Winnett's reading of the text states that he still keeps on translating ḏn as This although this interpretation had been rejected in my 1967 Thamudic ... There cannot be any doubt that O'Connor read the whole paragraph. He nonetheless still translates ḏn as "This" (!) without the slightest mention of my remark although he is - to say the least - unacquainted with Thamudic. This is another illustration of O'Connor's policy of hiding an information which he dislikes (cf. already above, p. 132). It is, however, most probable that, in the present case, his action was motivated by the political reason of flattering A. Naveh (cf. below, pp. 137 and 142). Finally, hlk is also known as a personal name in Thamudic JsaT 336.

c - The preposition fy is written as in Arabic, but the preposition f- is well-known in Thamudic; e.g., in the often-used expression wdd f- Love [is] in; e.g., But 197, Hu 7, 85+86/1, etc. For F.V. Winnett's silly interpretation of that expression as "He loves the mouth of" (cf. Winnett-Reed, l.c., pp. 74-75), cf. my review in JAOS, 92 (1972), pp. 526-527, and already my Thamudic ..., pp. 12 and 45-46. Therefore, contrary to all preceding authors, fy must be classified as Thamudic with the Arabic plain writing.

The tally of the various items of the text may thus be presented as follows:

Aramaic: 13: br, brt, dnh, hlk, yrh, mʾh, mry, ʿlmʾ, šnʾ, šnt, štyn, tmwz and tryn;

Aramaic-Arabic(X): 6: dy, hy, mn (prep.), mn (pron.), ftḥ and qbr;

Aramaic-Arabic: 2: ʾlḥgrw and ʾlqbrw;

Aramaic-Thamudic: 8: ʾm, b, -h (pron.), kʿbw, l-, ʿbdmnwtw, ʿly and rqwš;

Arabic(X): 4: dʾ, wld, ḥšy and ṣnʿ;

Thamudic: 2: ḥrtt and lʿn; and

Thamudic-Arabic(X): 1: fy.

Note: "Arabic(X)" means "Arabic words not found in Thamudic so far."

The preceding inventory puts the theory of Starcky, Müller, O'Connor, Healey and Smith in a very different light and shows how mistaken the pan-classical Arabic partisans and their followers who exclude JsaN 17 from Nabataean are.

5 - O'Connor's palaeographical ineptitude is showed by his use of either texts.

a - The Thamudic text.

As an appetizer to what follows, it may be useful to note that the so-called master of minutiose information (cf. above, p. 132) seems unable (cf. also above, p. 134) to provide his reader with the Euting and Huber sigla of JsaT 1 (p.221, fn 44, ll. 1-2); for those missing numbers and references, cf. above, p. 106.

The extent of O'Connor so-called study - it might be much more appropriate to speak of travesty or betrayal - is characterized by his long statement (p. 221B, fn 44) proudly announcing that

"I have at my disposal a photograph made by D.F. Graf, which does not offer material improvements to the reading of the text; Graf informs me that the text is angled in such a way that it is exceptionally difficult to photograph" (ll. 11-16).

The 1st half of his pronunciamento proves how erroneous is his evaluation of the material and especially of present-day photos even to the point of being out of his own sense: he is not even aware of what plain common sense dictates, viz the almost 90-year-old Jaussen-Savignac's documentation is BY FAR THE OBVIOUSLY BEST material to work with (cf. already above, p. 109). After that come Winnett-Reed's photos and notes because they were taken in late spring of 1962, thus more than 33 years ago. Yet, to the reader's distress and bewilderment and O'Connor's shame, Winnett-Reed's photo (cf. Ancient Records ..., pl. 33, no. 91, upper right corner) is never referred to by the writer who apparently did not care about or even - horribile dictu! - about the photos of Jaussen-Savignac's squeezes! It must be repeated and strongly emphasized that present-day photos C A N N O T ascertain the reading of the texts because they cannot but show how much the weather has during all those years damaged the letters, altered the appearance of the rock and, consequently, rendered the reading of the letters more difficult instead

of easier. Furthermore, a simple glance at Winnett-Reed's photo would also have shown O'Connor with accuracy the respective position of JsaN 17 and JsaT 1. The photos of Jaussen-Savignac's 1907 squeeze of JsaT 1 now at my disposal (cf. above, p. 105), which fortunately also contain the letters of the right side of JsaN 17 provide the following information which even Healey does not give although he claims having made the "examination of the rock-face" (p. 79, l. 10; cf. also above, pp. 107 and 117), - of course, it all depends on what he examined, the edges, the ridges, the crevices, etc -; that information was definitely not expected from amateurish Graf. That information reads as follows: the distance between the upper n of JsaT 1 and the initial d of JsaN 17/1 is 6.65 cm; and between the t of JsaT 1 and the initial d of JsaN 17/8 16.25 cm; and the height of JsaT 1 is 92.9 cm.

Thus, the tall, isosceles trapezium between the 2 texts is 92.9 cm high with 2 parallel sides, above and below, measuring respectively 6.65 and 16.25 cm. It is immediately obvious that Healey's distorted sketch makes JsaT 1 to run perfectly parallel to JsaN 17 (cf. already above, p. 109), and also that the bottom letter of JsaT 1 is below the level of d, the initial of JsaN 17/8 instead of being in the axis of its lower half! How can a writer unable to see the difference between a rectangle and an isosceles trapezium more than one yard high dare to speak ex cathedra of ... letter forms? It is now clear why Healey was unable or unwilling to see the difference between d and t and the exact form of the last w of l.9 (cf. already above, pp. 113-114 and 120, respectively). Here follow the measurements of 3 more letters:

in JsaT 1, the initial d: 5 x 3.25 cm;

the upper n: 11.25 x 2.15 cm; and

in JsaN 17/1, the initial d: 7.25 x 1 cm.

Then, O'Connor refers to F.V. Winnett's A Study ..., "p. 38" (p. 221B, l.23), where he copied his text, viz "zn rqš bnt ʿbdmnt 'This is Raqash, daughter of ʿAbdmanāt" (p. 221, ll. 20-21; but "Raqāsh" in ll. 16 and 17 of the same page). O'Connor who, as already stated above (p. 132), drowns his reader in his references, fails to refer to pl. VI, upper right corner, of the 1937 Winnett's booklet where the author reproduces his own version of the facsimile of the text. As far as O'Connor is concerned, there was no study on the text after Winnett's booklet which assuredly must have said the very last word about it. There is no reference to my 1967 Thamudic ..., p. 15, where I used unpublished photos of unpublished squeezes made by Jaussen-Savignac in 1907; cf. above, p. 109; and he is above all strangely interested in Jaussen-Savignac's facsimile and especially in his apparent luminary J. Naveh (fn 44, ll. 7-10) whom he reports to have reproduced that facsimile in 2 publications and published "two new Thamudic texts and a

discussion of gn" (ll. 8-9). O'Connor manifests a whimsical order of priority when paying so much attention to a manifestly inaccurate facsimile while disregarding Winnett-Reed's 1964 photo of the text (cf. above, p. 136). Naveh is dealt with below. Healey's reading of the initial letter is erroneous:

it is ḏ = H , not "z" = T (cf. my Thamudic ..., pp. 6-8).

Furthermore, blinded, as he is, by his sheepish attitude toward Winnett, O'Connor does not even see how irrational it is to state that "This is Raqah," who was a woman ..., regardless of the meaning given to "This." But he does not give any as if there was no problem. Of course, he might have thought - if by any chance he saw something - that the translation must be right since Winnett authored it!!! dn here is a noun, tomb; cf. already above, pp. 110, 115 and 135. Thus, the text JsaT 1 may be read as follows:

"Tomb of Raqūš, the daughter of ʿAbdmanāt."

b - The Nabataean text.

(1) dnh. — It is quite astounding that so much confusion exists in the reading of that masculine demonstrative pronoun. O'Connor repeats W. Diem's erroneous (cf. above, pp. 113-114) opinion according to which "either reading is palaeographically possible" (p. 222, pp. 14-15). Apparently, O'Connor cannot see by himself or does not want to see for political reasons or - which is the same in other words - he needs the protecting shield or friendship of all authors involved for — although retaining dnh as certain is his transliteration (p. 222, l.11) without giving any reason and, therefore, it is impossible to know whether such reason exists or whether it is a mere political choice to please some friends — he nonetheless considers the possibility of th being the correct reading: "If th is the preferred reading" (p. 223, l.3), AS IF the correct reading of a word were the result of a free choice, and he was saying to each of his friends: "Regardless of your choice - don't forget it, please! - I am on your side."

(2) qbrw. — O'Connor agrees with W. Diem who "has shown most recently" that the Nabataean ending -w was "designed to mark elements as (in some sense) non-Aramaic" (p. 223, ll. 6-7). While repeating the above solution which is particularly vague and abstruse, O'Connor's memory seems to be very short indeed because, 6 pages before (p. 217, l.7), he emphasizes that "the Aramaic is qbr" and 4 pages later (p. 227,l.21), he labels "qbr 'grave'" as "common"!!! Furthermore, O'Connor still reflecting on the final -w refers in fn 52 (p. 223A) to Cantineau, l.c., "vol. 1, p. 61." This erroneous reference which deals with 2 kinds of pronouns, must be corrected to II, pp. 165-169: "les noms propres en -w." Cf. above, p. 108, for Müller on the question of the -w ending.

(3) dnh qbrw gnʿh kʿbw: O'Connor spends 45 lines (pp. 223-224) to what he calls the "three competing interpretations" (p. 223, l.14). But the protagonists of the 1st 2 hypotheses refer to the same structural expression but with the relative pronoun dy interpolated before the verb, viz an entirely different pericope. The present formula is characterized by an asyndetic verbal proposition following a nominal attribute in the absolute state which is introduced by the demonstrative pronoun. The translation may thus be: This [is] the tomb, Kaʿab has made it =

This [is] the tomb which Kaʿab has made (cf. above, p. 129).

6 - O'Connor's swan song. — The object of O'Connor's paper is twofold as far as this writing is concerned (cf. above, pp. 129-131), viz to prove not only that Nabataean JsaN 17 is Arabic but also that Cantineau's approach to the text is erroneous.

a - Unable or unwilling to see the difference between right and wrong reading in Liḥyanite, but most anxious to speak his own mind for the apparent reason of boosting his theory on JsaN 17, O'Connor does not hesitate to endorse A.F.L. Beeston's travesty on JsaL 71 = JaL 118 (cf. above, p. 122) on p. 222, fn 46, ll.15-18. This is yet another worthless and shameful political move.

Resolute on terminating his paper in great style, O'Connor presents his own strongly partisan commentary (what else could he do?) of his statement of p. 222, ll.4-5 (cf. above, pp. 129-130) as follows:

> [A] "It is manifest that the Raqāsh Epitaph is closer to being a polyglot puzzle than Nabatean plain text. [B] It is eccentric enough to demand being treated on its own terms, [C] without being allowed to irregularize [sic] Nabatean grammar on a regular basis" (the divisions are mine; p. 227, ll. 31-33).

Ad [A]. — O'Connor must be congratulated for having successfully broken into a door already wide opened by the editors themselves who pointed out, almost 80 years before his verdict, the special character of the Nabataean text JsaN 17 and, therefore, there has never been any question of a "Nabatean plain text." O'Connor's mention of this expression, however, is nothing but a political trick to confuse the question because it brings to light a fact which even the editors plainly excluded from the start. This O'Connor's move can hardly be understood outside his frame calling for a "puzzle" (cf. immediately below). Furthermore, continuing in the same rut of exaggerating a situation in order to prove his point, O'Connor now speaks of "a polyglot" inscription although he has repeatedly talked of "a bilingual Nabatean-Thamudic text" (cf. above, pp. 132-133). According to normal understanding the adjective "polyglot," albeit indefinite in number, immediately suggests a much more diversified complexity than the adjective "bilingual." The use of that adjective, therefore, voluntarily distorts the whole image. O'Connor is nonetheless right in speaking of a "puzzle" because his voluntary ignorance of Thamudic, the native language of the author, and his voluntary substitution of Arabic, a language

foreign to the same author, cannot but lead to a fundamental, blundering misunderstanding which still cannot justify the word "polyglot" since O'Connor admits only Nabataean and Arabic; and so the situation merely becomes a "puzzle" ... for him! Cf. above, p. 130 for another aspect of O'Connor's "puzzle," the Cantineau-Starcky's case.

Ad [B]. — What is the meaning of both "eccentric enough" and "being treated on its own terms"? Even the smallest of the inscriptions containing only a simple identity card must be "treated on its own terms" because each one is liable to present particularities moulded by the individuality of each author. Nevertheless, it must still be analyzed within the general frame of one language. What is "eccentric" and downright wrong is indeed the stubborn narrow-mindedness of those authors who, although acknowledging the author of the text as Thamudaean, nevertheless exclude not only his native language from all consideration because of their voluntary simplistic preconception of their pan-classical Arabism, but also Kaʿab's deliberate will of writing a Nabataean inscription, JsaN 17, using his knowledge of Nabataean.

Ad [C]. — O'Connor unfortunately does not put any name to the so-called intent "to irregularize Nabatean grammar on a regular basis." Who wanted or wants to do it? But, if it is O'Connor's intention to suggest that Nabataean JsaN 17 should be dealt with separately, why did he not explain how it should be done, within what kind of general framework, and what name or siglum should henceforth be given to the text in accordance to his pompous oracle? Cf. above, p. 133, for a possible solution to the last question. It is indeed quite easy to claim having found a way out of a self-made "puzzle;" but to take the lead and show how to go about it is something else. Fortunately, the case is entirely different: ordered by a "Nabataeanizing" Thamudaean JsaN 17 represents, not a special dialect, but simply the language of an individual Thamudaean, whether Kaʿab himself or one of his close friends acting on his behalf; and the place where its characteristics must be described and analyzed still is the Nabataean grammar because the text, written in Nabataean letters, has an imposing Nabataean frame and flavor; and our knowledge of Thamudic remains too poor to have even a fair knowledge of its vocabulary. The least a true researcher can do is indeed to respectfully acknowledge the little that is presently known of Thamudic instead of using the imbecile trick of sweeping it under the rug by ignoring it and burying it in an all-consuming pan-classical Arabic; in this, unfortunately enough, all recent authors have been most successful.

b – There remains now for O'Connor to itemize the effects of his missile (cf. above, p. 129) not only to make sure that the reader is fully aware of the idols brought down by his blast but especially to give himself a hardy pat on the back in well-deserved congratulations for a job well-done. Only 5 lines before closing his paper, he writes this:

> [A] "As we have seen, Cantineau's versions of those notions led him astray – [B] the Arabic loan vocabulary is smaller and more specialized than he was led to imagine, [C] and the Raqāsh Epitaph clarifies neither the grammar nor history of Nabatean – [D] and we must be careful not to be similarly misled" (p. 229, ll. 4-8; the divisions are mine).

Ad [A]. – It has in all honesty to be said that O'Connor never showed the reader where Cantineau left the path of righteousness; on the contrary, O'Connor himself declared that he was going to "draw chiefly on Cantineau's own materials" (p. 215,l.3) and he, himself, concurs with W. Diem who acknowledges that "Cantineau is quite complete" (p. 215, fn 10, l.2). Therefore, how dares O'Connor say that Cantineau's "notions led him astray"?

Ad [B]. – Cf. already above, p. 131. The material published AFTER Cantineau's 1930 book supplied a great deal of information to be added – to be sure – to the documentation available to him; this is quite normal. However, O'Connor's phrasaeology sounds very much like comparing the present situation with Cantineau's to the effect of belittling the latter. If true, O'Connor would not be more unfair. Here follows a clear example of O'Connor' unfairness. He writes that

> "The fact that it is among the latest substantial Nabatean texts and the latest known to him led Cantineau to see it as only a step away from al-Namārah" (p. 222, ll. 5-7). It is perfectly amazing to see O'Connor untruly using here a picturesque expression, "as only a step away" although he based all his diatribe against Cantineau on similar expressions! One could easily tell O'Connor that "there is no flight of stairs with Nabatean 'only a step away from' Arabic" parallelly to his own phrase: "There is no trajectory of Nabatean 'turning into' Arabic" (cf. above, p. 133). Furthermore, O'Connor distorts the facts when claiming that JsaN 17 was "the latest known to him." How is this possible? O'Connor's text refers to his fn 47, which starts with these words:

> "In fact the chronological horizon around J17 can be filled in"

followed by the references to 4 (four) texts, viz

one of "266-67" A.D., published in 1967;
two of "306-7" A.D.: JsaN 386 and
CIS N 333; and
one of "356" A.D.: the Mâwîyat epitaph, published in 1968.

The 2 extreme references were published more than 30 years after Cantineau, while the 2 others of 306 A.D. were well-known to Cantineau, p. 22, and JsaN 386 is even reproduced in II, pp. 41-42, and the end of Cantineau's commentary on the text summarizes the chronology quite accurately, viz

"La date , 201 de la Province, 307 de notre ère, fait de cette inscription, avec celle du Corpus n° 333, le texte nabatéen le plus récent. Elle est cependant moins chargée d'arabisme que l'inscription J.17" (cf. II, p. 42).

This is another specimen of Cantineau's matter-of-fact style where O'Connor claims to have found "a streak of romanticism" (cf. above, pp. 130-131).

Ad [C]. - Nobody - and certainly not Cantineau - ever thought or imagined that JsaN 17 might clarify the grammar and history of Nabataean because its author obviously was not Nabataean by birth, and the text is not historical at all. But O'Connor did not fare better than his predecessors, including J. Starcky, by paying no attention whatsoever to both Thamudic, the mother tongue of the author himself, and the "Nabataeanized" nominal forms and, what is much worst, by casting them off to oblivion inside an omnivorous pan-classical Arabic.

Ad [D]. - The critics scattered in the preceding pages suggest as the best answer to O'Connor's unwelcome, patronizing statement that should in any event be restricted to his classroom and, above all,...to himself, these 2 well-known quotations: Medice, cura teipsum, and Reddite quae sunt Caesaris, Caesari et quae sunt Thamudic to the Thamudaeans, instead of consigning them to the pan-classical Arabic melting pot.

Addendum 1 - J. Naveh and North-Arabian.

A - Naveh and Thamudic.

O'Connor misguided reliance on amateurish Naveh's work in Thamudic(cf. above, pp. above, pp. 135 and 138) is a political move proving his incompetence in Thamudic. Naveh's work is untrustworthy, being written by an Aramaist adjusting palaeography to suit himself while comfortably riding on a well-cushioned North-Arabic cart pulled by the Winnett - van den Branden - Harding troika. Naveh published so far - at least to my knowledge - 9 Thamudic texts which must be republished.

NaT 1 (cf. IEJ, 24 [1974], pp. 79-83 and pl. 12-13, in collaboration with E. Stern): an inscription "inside the bowl, below its rim and parallel to it" (p. 81). The diameter of the bowl is 7.2 cm in pl. 13A, and since it actually is "9 cm" (p. 79), the scale of pl. 13A, which remains unexplained, is composed of 1-cm each.

lḫdd bn msk dhn To Ḥudâd, son of Mâsik,[who] was intelligent.

This text is yet another example of the Thamudic rule of very often omitting the relative pronoun (cf. already above, p. 115, and also below, NaT 3 in p. 143).- ḫdd and msk, as in the edition. - The last 3 letters dhn are read by Naveh "zdm[ ]" (p. 81). This reading is erroneous: the 1st letter, I or H is d (cf. already above, p. 138), the 2nd letter manifestly is h, and the last is the almost right half of n. Furthermore, Naveh claims that "We may assume that there were two or three more letters on the (now missing) edge" (p. 81). This statement remains without any proof or justification but is

IMPOSED by Naveh's PRECONCEIVED interpretation of the text. It should also be pointed out that Stern, who is responsible for the description of the vessel (pp. 79-81), fails to even mention the circular shape of the upper cup. No reason can justify the photo of pl. 13B not showing the whole of the cup: the bottom is cut off!; and this makes it more difficult to draw the whole rim. At any rate, even the printed photo shows that no letter is missing because the bottom end of it should still be visible on the cup, and there is none to be seen. Finally, Naveh's reading "zdm[ ]" is most frantically interpreted as z[y]d + mnt or better still as "Zdm[r²n]" because "Mr²n is known by the name ʿbdmr²n from a recently published Aramaic ostracon found near Raphia;" and "in accordance with Winnett's translation of the Lihyanite [sic] text JS 45" (both quotations from p. 82), viz zddġbt being translated "may Dhu Ghabat bless him"(cf. F.V. Winnett, A Study ..., p. 15), Naveh proceeds claiming that "Zedma[r²an]" may be understood as "May Ma-[rna]bless him" (p. 82). What an undescribable bouillabaisse worthy of C. Robin(cf.above, p. 95)! In order to give some kind of meaning to his fallacious interpretation, Winnett had to invent and insert into the name the personal pronoun, -h-, viz his translation of the name of JsaL 45 demands the reading of the inexistent "zdhdġbt" instead of zddġbt. But, Winnett's commentary does not even mention his betrayal of the original, and Naveh, who sheepishly copied both the text and Winnett's translation (p. 82 and fn 11), failed to mention Winnett's imposture! And, writers of that caliber are called scholars or researchers!!! The Liḥyanite personal name zddġbt (cf. already W . Caskel, Lihyan ... [1954], pp. 111 and 154B, a testimony which Naveh shamefully spurns) is parallelled by zd²krw (JaL 16 c/1), zdhn²ktb (JsaL 78/1), zdwd (JaL 77/4-5), zdḥmm (Stiehl 24/1), zdḥm (JaL 90 c/1), zdhrg (JaL 92 b/1), etc and, on the other hand, ²ldġbt (JaL 79b), bldġbt (JaL 49), mr²dġbt (JaL 107a) and ʿbddġbt (JaL 109 a/1) = ʿbdġbt (JaL 93a). The personal name zddġbt is not listed in G.L. Harding, An Index ... Toronto, 1971, pp. 298b, 304, 915 and 937B; the author surely endorses Winnett's interpretation.

NaT 2-5 (cf. EI, 12 [1975], pp. 129-131 and pl. 17, nos. 1 and 2, and English summary on p. 123*A): 4 vertical lines to be read, as usual, from top to bottom; the texts are represented horizontally in pl. 27.

NaT 2: dn ʿmr bn ʿt bʿnq ḫlbt Has perished ʿAmr, son of ʿAtt, at the col of Ḫalbat.
ʿmr and ʿt, as in the edition; cf. above, p. 125.

NaT 3: dn ḥmr ²br Has perished Ḥimâr [who] was in a good state.
Cf. above, p. 142, the initial note on NaT 1.

NaT 4: dn ʿqlt Has perished ʿAqlat.

NaT 5: aslant to NaT 4 in pl. 27, no. 2, top; not mentioned in Naveh's paper; the 1st 3 letters only are readable: ḫly...

For the dn inscriptions, cf. above, p. 138.

B - Naveh and Safaitic, Thamudic (cf. EI, 14 [1978], pp. 178-182, pl. 4-6, summary in p. 129*B): 5 inscriptions engraved on the outside of 3 stone bowls; all letters are visible on the 11 photos; the end of NaT 7, however, is not perfectly legible in pl. 6, nos. 1 and 2.

NaS 1: the end is read "qr //" (p. 178A); "slm wqr//" is translated "(give her) peace and coolness(?)" (p. 178A; also p. 129*B). This erroneous reading must be corrected to the well-known expression slm/wqbll peace and welcome!; cf., e.g., JaS 71a and commentary in "Safaitic Inscriptions from the Country of ʿArʿar and Haʾs al-ʿAnāniyah," in F. Altheim and R. Stiehl, Christentum am roten Meer, I, Berlin, 1971, p. 66, and my Safaitic Notes, Washington, 1970, p. 46, note 35. Furthermore, Naveh's reading of "r" instead of "b" is all the more incomprehensible that b of qbll is immediately below b of ʿbd in pl. 1, no. 3: the 2 letters could hardly be more identical. Finally, it is the commentary on his erroneous "qr //" that Naveh refers to another of his inventions in the last word of NaT 1 (cf. above, p. 143).

NaT 6 a-c are engraved on the 2nd bowl along with 2 short Nabataean texts (p. 180B and 129*B); NaN 1 was written before NaT 6a which was traced to its right, and NaN 2, the name of the author of NaT 6a, is found just below the beginning of that inscription.

NaT 6a: the last word is read "fṙy" in p. 180B (but "PRY" in p. 129*B; for the transliteration of "p" and "f," cf. already above, p. 110). The 2 r's of the text disprove such a reading. Since the name "fby" is unexplainable, Naveh misread the name as "fṙy". The reading fby means And he stopped [here]. For the verb by to stop at a place, e.g., LP 243 (cf. my paper in Orientalia, 36 [1967], pp. 161-162) = bwy, e.g., CIS 1828 (cf. my Safaitic ..., p. 66, note 81). Here again, Naveh's reading of "r" instead of "b" is all the more incomprehensible (cf. already above, this page, NaS 1) that b of fby is immediately below r of rks in pl. 2, no. 2: the difference between the 2 letters could not be more striking. The preceding case and that of Naveh's "r" of NaS 1 (cf. above, this page) are 2 undeniable examples of Naveh's voluntary mistreatment of palaeography to suit himself, viz to hide his ignorance. Furthermore, Naveh refers his "fṙy" to G.L. Harding, An Index ..., p. 467 (cf. p. 181A and fn 21). The very last phrase of my lengthy review of G.L. Harding, An Index ..., in my Miscellanées ..., II (1971), pp. 93-150, reads as follows: "Ici, comme dans les cas des copies de Philby, il faudra prolonger la liste des corrections lorsqu'un auteur utilisera l'index en guise de référence" (p. 150).

Apparently Naveh failed - as many others - to give heed to the preceding warning. The text referred to by Naveh is WiTh 63 published by Winnett-Reed, l.c., p. 110, where the name is read "zfry"! These 2 readings were corrected to fny in my Miscellanées ..., V (1974), p. 82, under Hu 513.

NaT 6b: below the 1st 3rd of NaT 6a and NaN 2: lmzrt bnt ʾfṣ, as in the edition (p. 180B) and "By MZRT daughter of ʾPṢ" (p. 129*B). Here again Naveh refers to Harding's book with - alas! - the same desastrous result.

For mzrt Mazrat, Naveh's fn 26 (p. 181A) refers to Harding, p. 543, where the reader finds mzr with the mention of Safaitic CIS 2086 and 5089.

In CIS 2086, which actually is 2086a, CIS reads "mz" which must be corrected to ʿml ʿĀmil (cf. my Safaitic ..., p. 61, note 69).

In CIS 5809, CIS has "m(sd)," an unacceptable correction of the original mrz Māriz, which is found in the 2 different copies of J.G. Wetzstein (cf. ZDMG, 30 [1876], p. 514, pl. I, no. IIa) and H. Grimme (Texte und Untersuchungen zur safanetisch-arabischen Religion, Paderborn, 1929, pl. XIII, no. 361). For mrz, cf. already CIS 1273 (cf. Miscellanées ..., IV [1973], p. 146, note 131).

For ʾfṣ ʾAfayṣ, Naveh refers to Harding, p. 58, where the reader finds "JS 56/2, doubtful. [Thamudic] TIJ 221, 426." Harding's reading of ʾfṣ in JsaL 56/2 is certain; cf. my Miscellanées ..., VII (1974), p. 145, note 151. Harding's reading of "ʾfṣ in HaLi 221 is based on his "ʾfṣ" of HaLi 426 where, however, the photo shows ʾft ʾAfatt; cf. Safaitic ft in CIS 4861; and hfṣ Hafayṣ is clear on the copy of HaLi 221.

NaT 6c: Naveh's interpretation of the last 3/4 of the text is comparable with F.V. Winnett's worst fanciful translations: it reads as follows: "lsryt wʾsnt mṭy lḥgrn (p. 180B) = "By SRYT. And he came this year to our region" (p. 129*B and 180B). "ʾsnt" is referred by Naveh to Wi 37 (p. 181A and fn 30); he could also have referred to WH 1906 because in both places Winnett considers the form as a singular although it is manifestly a plural (cf., respectively, my Minaean Inscriptions Published as Lihyanite, Washington, 1968, p. 123, note 69, and my Miscellanées ..., XIII [1983], p. 215). Finally, the "s" of Naveh's reading is an error instead of h. - mṭy is normally used in the passive voice, to be delayed for a long time. - ḥgrn is a personal name (e.g., JaS 175). Naveh takes the -n ending as the 1st plural personal pronoun (the same fictitious -n is taken as the singular by Knauf in Thamudic; cf. above, p. 116). What a nonsense for a plural personal pronoun to refer to persons not even otherwise mentioned in a text engraved by one individual only! And where does the meaning of "region" for ḥgr come from? Finally, to favor his craze Naveh places the temporal complement at the very beginning of the sentence! The text may be read as follows:

lsryt w²hnt mṭy lḥgrn

By Saryat. And ²Ahawnat was delayed [here] a long time for Ḥagrân.

NaT 7: on the 3rd bowl. - For Naveh's transliteration, cf. p. 181B: "l²bmlk wdkrt lt hms wmlḥ ws'lh wkrt w²'ṣ w²sd whmtn w'wd w²dnl wms'd ...;" and English translation in p. 129B reads:

"By Abumalik. May Allat remember HMS and MLḤ and Shai' allah and ...!"

The text may be read as follows:

l²bmlk wdkrt lt hml wmlḥ wl'lh wkrt w²'ṣ w²sd whmtn w'wd w²d wdnb wms'd wḥl²nlt

By ²Abdmalik. And Lat has remembered Ḥâmil and Mulâḥ and Il'âlih and Karrat and ²A'awṣ and ²Asad and Ḥamṭân and 'Awad and Danib and Mas'ûd and Ha'awnlat.

For dkrt ..., e.g., JaT 131 a/1; cf. above, pp. 118-119. - hml (instead of Naveh's "hms"), e.g., JaT 103 h/2. - mlḥ, e.g., JaT 38 b/2. - l'lh (instead of Naveh's "s'lh"), e.g.,JaT 118 a/1. - krt, e.g., JaT 32. - ²'ṣ, cf. Safaitic, e.g., IM 23917a, and ²'ṣt in WH 904 (cf. my Miscellanées ..., XIII [1983], pp. 202-203). - ²sd, e.g., Hu 282. - ḥmtn, Naveh's commentary declares the name to be unknown; cf. Ḥadrami ḥmt in my paper of BASOR, No. 172, December 1963, p. 44: t instead of ś; cf. Arabic ḥamis "ardent, eager." - 'wd, cf. JoDD 6. - dnb (instead of Naveh's "²dnl"), cf. Ramm 17. - ms'd, cf. Safaitic, e.g., CIS 409, and Thamudic ms'dt of JsaT 269. - wh'nlt (not read by Naveh); for h'n, cf. Ḥâ²il 196.

Addendum 2 - The end of Safaitic ClAS 661 (cf. V. Clark, A Study of New Safaitic Inscriptions from Jordan, Ann Arbor, 1983, pp. 326-327 and facsimile in p. 557) and E.A. Knauf.

Healey's misreading of the initial letter of JsaN 17/1 was the consequence of his failure to study the palaeography of the Nabataean letters d and t and this, in turn, had to be avoided at all cost in order not to upset the previous imagination of a diacritical dot above d. Similarly, bent on finding a reference to Nabataean, the schemer E.A. Knauf excluded right off my rational reading of the last word of ClAS 661 without studying the palaeography of the letter involved, and this had to be avoided at all cost in order to maintain his preconceived scheme on nbṭ; my reading simply thwarted his dream and had to be set aside! My Miscellanées ... (XIII [1983], pp. 187-188) have already given some examples of Knauf's nonsensical work always aimed at proving his scheme - whatever it may be - regardless of palaeography and/or good common sense.

The end of ClAS 661 is read "hnbṭ[y] the Nabataean" in the edition (p. 326). My lengthy review of Clark's thesis in my Miscellanées ..., XIII [1983], pp. 2-113) denounces the editor's illogical attitude and suggests the following reading:

"hn bṭl Baṭal rested [here]" (p. 70).

As fully expected, the newspaporial droll Knauf rejects my preceding interpretation as

"aberrant" (cf. M.M. Ibrahim ed., Arabian Studies in Honour of Mahmoud Ghul..., Wiesbaden, (1989) 1990, p. 57 and fn 8). Coming from Knauf, any such condemnation is quite a compliment. Of course, Knauf is so hypnothized by his verbose kalâm which is a living illustration of the proverb "Les idées peuvent manquer, mais jamais la bouche," [the original word is much more expressive] that, on the one hand, he even published twice the very same pamphlet, viz the 133 pages of his 1985 Ismael (= text, footnotes, "Nachwort," "Abkürzungsverzeichnis" and "Register") are reproduced again anastatically under the same title in 1989 (4 years later!), and on the other hand, the rhythm of his own verbal discharge deludes him with the assurance that any proof is manifestly superfluous. The present case is not an exception (cf. already above, pp. 115-116): Knauf does not give any reason or even discuss the palaeography of that special form of l in the Clark's collection; nor is he even honest enough to inform his reader that the editor's reading is "hnbṭ[y]" (cf. already above, p. 146) instead of his "hnbṭy," as he puts it, the parentheses being eliminated by the sheer impact of his own preconceived wild vision. Summarily, Knauf performed exactly as expected; cf. already above, pp. 115-116. Furthermore, M.C.A. Macdonald gives another example of his peculiar reasoning method (cf. ZDPV, 107 pp. 106-107) when, although distorting the "hnbṭ[y]" of the edition to his "h-nbṭy" (p. 106 and fn 39), he brushes aside whatever value he gives to his "-" and considers the final "[y]" as absolutely certain (fn 41, end) since he speaks of "the man mentioned in CSNS 661 clearly did" [my emphasis], and of "the nisba" (l.c.). Finally, equally unjustifiable is the case of The New Encyclopaedia of Islam, VII (1992), p. 835A, where D.F. Graf maintains the reading "hnbṭy" of ClAS 661 in submission to Knauf's error, without giving any explanation or justification or without even mentioning the contrary reading of my Miscellanées! Two final notes on this last contribution: in p. 835A, l. 31, correct "unpubl." to "Ann Arbor, 1983" (cf. already above, p. 146) and p. 835B, l. 21, correct "déserte" to "désastre."

The letter in question is a bar (straight or slightly curved) with an oblong blotch located at one extremity of the bar, either alongside or atop the staff. This blotch is not to be confused with a rounded or triangular design which characterizes the letter y. Clark did NOT study the form of the letter; his charter (pp. 70-71) showing "various classifications" (p. 69) is a simple eyewash, and he reads that letter differently according - no doubt - to the whims of his various moods. Here follow a few notes taken during a limited probe: Clark reads that very letter as

"n" in ClAS 555: "dkrn" (for the end of this text, cf. my Miscellanées ..., p. 64);

"r" in ClAS 722 (p. 338 and 560): "²ḥjr" instead of ²ḥjl; or

"l" (most often), e.g., in ClAS 559 (pp. 300 and 552): "dhl" instead of dhy; for this text, cf. my Miscellanees ..., l.c., p. 64;
568 (pp. 303 and 553): "w'l" instead of w'y;
706 (pp. 335 and 559): "g'l" instead of g'y; etc.

Even a plain, clear y is read "l;" e.g., ClAS 578 (pp. 304 and 553): "glm" instead of gym; the "y" of ClAS 603 is irrelevant: the identity of the 2 persons cannot be ascertained. A blotch is normally caused by a bic about to empty.

Addendum 3 - J.F. Healey and A.S. al-Theeb's thesis.

J.F. Healey's scientific method is illustrated by A.S. al-Theeb's (Ar الذيب and also الزيب in التصوير, 7(2) [1413/1992], pp. 217-254, every page) thesis entitled Aramaic and Nabataean Inscriptions from North-West Saudi Arabia (Riyadh, 1414/1993), viz his 1989 Ph.D. thesis at the Durham University (p. II). The author's 1st paragraph of his "Acknowledgements" (p.V) starts as follows:

"I wish to express my deep gratitude and sincere appreciation to my supervisor, Dr J. Healey, for his encouragement, guidance, advice and patience in helping to clarify the presentation of my arguments ..." (ll. 2-4).

The irregularities and errors in names, titles, reasonings and documentation are so numerous that I have NEVER seen the like before; they vividly illustrate the MONSTROUS FIASCO of J.F. Healey's supervision. Three short and glaring examples suffice here.

P. 346: to the 2nd entry of T. Nöldeke, which contains 2 errors, the reader must add the mention of the tome, pagination and plates; and most incredibly, the name of the institution with 2 new errors becomes the title itself of the book in p. XIII!

P. 352: the title of "Zarins, J., ..." (pp.76-83) with again 2 errors, has the 4 authors wrong. al-Theeb copied those names in Atlal, 7 (1403/1983), p. 3, where they belong to the 2nd article listed on the same page. For those names, 3, instead of 4, cf. Atlal, l.c., p. 76.

P. 73. - P.K. Hitti on the Nabataeans' ethnic origin. - al-Theeb attributes to Hitti an opinion contrary to all his writings (!) because he blindly copied it from J. I. Lawlord, The Nabataeans in Historical Perspective (Grand Rapids, 1974), p. 27, where "The Arab Heritage" (edited by F.N. Faris, Princeton, 1946) is erroneously attributed to P.K. Hitti: this book is composed as 9 essays , the 1st of which (pp. 1-24) is by Hitti. The incriminated passage comes from p. 36, ll. 11-13, in G. Levi Della Vida's contribution on "Pre-Islamic Arabia" of pp. 25-57.

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June 21, 1993
with later additions

V

Geraldine King
on the Thamudic inscriptions
of wadis Judayid and Matah
(southwestern Jordan)

A - The graffiti of wâdî Judayid; siglum KiWJ.

Geraldine King's "Preliminary Report" on the wâdî Judayid Thamudic inscriptions and petroglyths (cf. ADAJ, 32 [1988], pp. 307-317 and 402-404 of pl. 49-51 with the running headline mentioning "G.M.H. King") is illustrated not only by facsimiles betraying a poor cursory study of the originals but also by across-the-board imaginary statements, e.g., her sketch of letters (p. 312) belongs to some hoepoe-inspired fanciness in the line of F.V.Winnett. She also fails to inform the reader that, inspired by F.V. Winnett's tradition which was embodied in the early eighties by W.J. Jobling's papers and which she had discarded in her previous article (cf. below, pp.154-end), she interprets the letters according to her own transitory or long-lived freaks, viz

^ { of KiWJ 1 (end) is read "r," but
  { of KiWJ 4 (beginning) is read "b;"

| { of KiWJ 7 is read both "l" and "š;"
| { of KiWJ 6 is read "š;"
| { of KiWJ 2 is read "l."

What a splendid weathercock in the likeness of the incomparable A.F.L. Beeston!

Cf. already my Miscellanées d'ancient arabe, Washington, XVI (1988), pp. 218-221.

How easy is the reading of an inscription when chosing the most convenient value? That unscientific method which had become routine with Winnett has been endorsed in England as proven by King's papers published with the strong support of the pan-classical Arabic fanatics. Finally, her selection of the texts is unjustified: it is not in keeping with the great importance given to the 2 main theses of the introduction and to the other points as well; viz the 2 unjustifiable stopgaps, KiWJ 8 and 9, are not mentioned explicitely or implicitely in pp. 312-313 which are some kind of sketchy summary of the whole collection; the wdd- and n(y)k- pericopes are listed (p. 313B) but not illustrated. This King's paper is in the line of her article on "Some Inscriptions from Wadi Matakh" (cf. M.M. Ibrahim ed. [[cf. above, p. 147, top]]; it is erroneously dated in "1988" in the present paper): the Matah texts published in it must be republished again from scratch because of too many fantasies in the facsimiles, decipherments and comments (their study may be found immediately after this section). As a rule, those fantasies are mere statements left without any proof or explanation because, as she is amply aware, those assertions are fully endorsed by her English friends in the same manner than C. Robin's

aerial expectorations are gulped down by his French admirers. But, when there is some attempt at justification, it is mere childish and one-sided. The basic difference between the 2 papers is an appalling craziness: in the 1990 paper which was written before the 1988 article, the letter ḏ, ṯ, etc are maintained in both the transliteration of the text and (when it belongs to a proper name) the translation as well. BUT they become ṯ, ḍ, etc, respectively, in the translation if they do not belong to proper names!!! Is it possible? It is as if research actually was run by impulsive lunacy.

My unsystematic study of King's paper is centered on the inscriptions themselves, none of which is interpreted adequately, and King's reading of the letters is correct only in KiWJ 10. When the reader learns that "1302 Thamudic inscriptions" (p. 309B) were recorded, he knows for sure that he has only to brace himself for another savage onslaught in the late nineties as he had to deal in the eighties with V.A.Clark's publications (cf. my reviews in my Miscellanées ..., XIII [1983], pp. 2-113 and XV [1987] pp. 84-123) and W.J. Jobing's articles (cf. my review in l.c., XVI [1988], pp. 160-250). Needless to say that neither King nor her English patronizing admirers never refer to those 3 reviews. Of course!

Summarily, King's own infatuation takes the precedence of the basics in research: such is the unescapable message of her paper.

I - King's partisanship.

King's partisanship is visible in every single page of her article. Here follow 4 examples.

a - Her commentary on her erroneous reading of "ʾmd" (cf. below, KiWJ 7) informs the reader that "the reference in HIn is a mistake" (p. 341B). No explanatory note of any kind is added. Her lack of honesty prevented her from informing the reader that the correction with the accurate reading is already found in my Miscellanées ..., XIII (1983) pp. 219 and 268A, a publication not even listed in her "Bibliography" of p. 316.

b - Another type of dishonesty is exemplified by the following case which is plain partisanship. E.A. Knauf's paper in Levant, 17 (1985), pp. 204-206, is listed in p.316. Here again King lacks the basic honesty of informing the reader that Knauf's paper is thoroughly rebuffed in my Miscellanées ..., XIV (1985), pp. 17-36; this publication also found no entry in her "Bibliography."

c - d - More on the question of bibliography.

F.V. Winnett's "1982 Studies in Thamudic, unpublished" (p. 317) was apparently distributed among his English friends. It appeared in JAOS, 117 (1987), pp. 239-244, and was refuted in my Miscellanées ..., XV (1987), pp. 124-191; this publication is another casualty of her "Bibliography." And finally in fn 5 (p. 309B), King refers to W.J. Job-

ling's 7 papers which are listed in her bibliography (p. 316). In fact those 7 papers and 5 (five) more papers from Jobling which are not mentioned in p. 316 were discussed and all their texts republished in my Miscellanées ..., XVI (1988), pp. 160-250 (cf. the preceding page).

II - King's parody of decipherment.

King's self-infatuation also induced her to make a show of her skill of deciphering photos, obviously aiming at demonstrating to the reader how qualified she really is for the job. Indeed she does it in p. 313, the page immediately preceding the start of her publication of the texts so that the reader would have no chance to forget her astounding fitness and feats.

a - P. 313, fn 14. - King claims that her reading of HaLi 295 as "wdd mr'lh ġlmt 'mr'lh loved a young girl' is fairly clear in the published photograph" and she describes what she claims to be the proof of her "d"'s and "ġ." Her statements are unquestionably accurate because the incompetent King, unable to distinguish the differences of shades and also the tracing of the letters, integrates in her reading scratches and designs in the rock.

(1) The most spectacular of them is a slightly aslant double curve the top of which touches the right end of l of mr'lh, and its bottom the upper left side of b; that feature, ʃ, became "ġ" for King!!!

(2) The 2 pronks of s, the 2nd letter, are distinct and quite clear, and the center of the whole letter, where the 2 pronks join together, is garbled by a circular natural design the color of which sharply contrasts with that of the letter, but that has totally obnubilated myopic King to the exclusion of anything else.

My reading of wsy mr'lh bn lmt Mar'allah, son of Lummat, has been consoled (cf. my Minnaean Inscriptions Published as Libyanite, Washington, 1968, pp. 82-83, note 30) still stands.

b - Fn 10 of p. 313A presents a shocking case of a distorted quote, of plagiarism and of palaeographical ineptitude; it may safely be singled out as a notable representative example of Geraldine King's vicious expertise.

King begins with "al-ḫawar, 'the third star ...' (Lane)," which is to be coupled with her statement on "Ar. al-'aḫwar, 'the planet Jupiter' (Lane)" (p. 314A, commentary on KiWJ 2); cf. already my Miscellanées ..., XVIII (1995), p. 156 on D.F. Graf.

First, King makes up Lane's quote on al-'aḫwar because Lane says: "al-'aḫwar A certain star (ʕ, ḫ) or Jupiter (ʕ, ḫ)" (p. 666C, ll. 44-45); thus, the expression "the planet" does not come from Lane!!! Secondly, she formally plagiarizes E. Littmann; here follow the 2 quotations one above the other for immediate visual comparison:

G. King: "al-'aḫwar, 'the planet Jupiter'," most used by King, cf. p. 307 and fn 2), p. 34, commentary on HaLi 323, where the above-quoted phrase is followed immediately by this: "Taim-'Aḫwar 'the servant of Jupiter'," and this phrase, in turn, is further repeated in p. 38, commentary on HaLi 393.

E. Littmann: "Aḫwar is the planet Jupiter" in G.L. Harding - E. Littmann, Some Thamudic Inscriptions from the Hashemite Kingdom of Jordan, Leiden, 1952 (a volume

King's second statement of the same fn 10 is to the effect that "the name tmlḫwr also occurs in" HaLi 434. Voluntarily hypnothized to blindness by her intent of proving her thesis which implies a Harding-Littmann's reading error, she inevitably took for granted what was necessary for her thesis, viz that there was only a single text there. But the unicity of the 2 texts is opposed by the size of the letters which is larger in HaLi 434b than in HaLi 434a; for the reading, cf. my Miscellanées ..., l.c.

III - The inscriptions.

Not only too many of King's readings are erroneous and her interpretations off track, but also her dishonesty (cf. above, this page for the 1st case of plagiarism, and below, p. 158 for the 2nd) shines again in her voluntary concealing of the most important part of the documentation on her major question of the compound names with ḫwr, viz

Lihyanite ḫwyr of JsaL 129/1,
ḫwr of JsaL 85/1,
Safaitic mḫwr in CIS, e.g., 4448, and
Thamudic mḫwr in HaLi 60.

It is difficult to explain how the world never heard of "little Jupiter" which would correspond to the diminutive ḫwyr!!! Furthermore, as shown above, King went out of her way to bring in HaLi 434 in order to posit her wrong reading of "tmlḫwr," and she also publishes KiWJ 2 with her so-called "'bd'ḫwr;" therefore, her list of compound names with ḫwr contains only ONE MORE NAME, viz "whb'ḫwr" (p. 312B). It is quite inexplicable to keep that single text out of her paper and to have instead retained 2 vulgar texts composed each with a short name, KiWJ 8 and 9.

King justifies her choice of WiWJ 4 as follows:

"the final letter is the sign of two concentric circles and the reading of it as ġ would be unacceptable" (p. 312B) =
"This is one of several texts that support the reading of the sign of two concentric circles as ḏ" (p. 314A-B).

It is equally unintelligible that she would not give at least another example of her erroneous reading of "ḏ," to which she gives so much emphasis in p. 312. What is she afraid of? Here again, any of those "several" gems is certainly more precious to her than any of the ordinary texts, KiWJ 8 and 9. But the 4-letter name of KiWJ 9 has the distin-

guished honor of being a typical example of King's workmanship; cf. below, p. 155. The following study of the texts does not itemize the corrections of King's readings or interpretations. There is no advantage to be gained from repeating those errors again.

KiWJ 1 (pp. 312 , 313A, 314A, 315 and pl. 50A).

King writes that "some further letters, a d and a t, are written in a different hand to the left of the text."

What a nice way for King to very quietly sweep under the rug 2 letters as if they did not exist! What a haughtiness to think that some Thamudic writer wasted his time writing 2 useless letters! Would those 2 letters stand alone, they could be interpreted as a graffito to be read from bottom to top, viz td ↑ Tayd; cf. Arabic tayd "lenitas, benignitas" (cf. G.W. Freytag). But, King keeps on distorting the truth by hiding from the reader the fact that those 2 letters are located exactly to the left of the last 2 letters of the main text, viz (left) d / t | d / t, (right)

and this fact proves that those 2 letters are corrections of the 2 others; indeed the 2 letters "d" and "t" are poorly traced. Thus, ḫrdt must be read instead of ḫrṭt (erroneously read "ḫrtt" by King). Furthermore, for the pericope dkrt lt, cf. my reply to Healey-Smith's paper, above p. 119. - The text may be read as follows:

wdkrt lt ᶜbd ḫrdt

And Lat has remembered the servant of Ḫardat.

For ḫrdt, cf. the verb ḫrd to separate oneself from the others; e.g., in Hu 350.

KiWJ 2 (pp. 312B, 314A, 315 and pl. 50B).

The text may be read as follows: lᶜbdh ḫwb By ᶜAbduh. He acted unlawfully. ᶜbdh, cf. Liḥyanite JsaL 380/2 and Safaitic CIS 4603. - ḫwb, cf. my Miscellanées ..., XIII (1983), pp. 114-115 and 262B.

KiWJ 3 (pp. 314B, 315 and pl. 50B).

King unites 2 manifestly different graffiti.

A (right), engraved as a half-ellipsis with rounded letter forms, and

B (left), a straight vertical line with angular forms; cf., e.g., the trapezium of g.

A - lġnn bn hn² By Ġunan, son of Hani². - For ġnn, cf. Safaitic ġn, e.g., LP 279, and ġnnn of Wi 83.

B - bn ddqm Has halted [here] Dûdaqim. - For ddqm, cf. Arabic daqama "fregit anteriores dentes" and daqam "noxa" (cf. G.W. Freytag).

KiWJ 4 (pp. 312B, 314A-B, 315 and pl. 50B).

lb ġg Has stopped [here] Ġawg.

lb, e.g., Hu 161a. - ġg, cf. Safaitic, e.g., ClAS 1153. - It is really pathetic and painful to see the biased self-conceit of King claiming that "the reading of it," viz "the sign of two concentric circles" "as g would be unacceptable" (p. 312B; cf. also above, pp. 153-154 for the equivalent statement of p. 314A-B). And it is equally pathetic and painful to see that, in her infatuation, "bġṭ is well-attested in Safaitic" (p. 314B) actually justifies her reading of "bġṭ" exclusive of any other reading. This inane reason is repeated over and over again in almost every following page.

KiWJ 5 (pp. 314B, 315 and pl. 50B).

lmltn By Maltân.

Cf. the verb mlt to flatter in Hu 402/2.

KiWJ 6 (pp. 313A, 314B, 315 and pl. 51B).

wd ᶜdl ṣtnn ᶜAdil has loved Ṣatnân.

wd, e.g., JaT 78a. - ᶜdl, JsaT 360. - ṣtnn, cf. Arabic ṣatta "to cry out, shout." The commentary (p. 314B) states that "it is the first attestation of the deity's name spelt with a d" (11.20-21); but translation and transliteration have both "dšry"!!! Who can tell what King might have had in mind except that her remark could (?!) have been intended for KiWJ 7? Who can fathom what a deranged mind is capable of thinking of?

KiWJ 7 (pp. 313B, 314B, 315 and pl. 50B).

hdl ry lᵓtd sᶜd Hadal brought water to ᵓAtad [who] was happy.

hdl, cf. Safaitic, e.g., JaS 150a. - ry l-, JsaT 197. - ᵓtd, apocopated form of ᵓtdw of JaS 57a. - sᶜd, e.g., Eut 404b. - A special note is needed here to denounce King's invention of a new formula created here to save her ignorance, viz she claims that "l here has a dative force" in the pericope "dšry l-"!!! She cannot even understand how irrational it is to refer a preposition to a nonexistent verb which is restored in her translation without any brackets or even parentheses (as she does in KiWJ 10 with her "it;" she most probably was not in the mood yet) as if the verb really existed!!!

KiWJ 8 (pp. 314B, 315 and pl. 51A).

lbnn By Banîn.

Cf., e.g., JaT 105f. King erroneously extends upward the right edge of the letter to read "k." The upper right angle of b is pointy, nothing else. - Because G.L. Harding, An Index ... (p. 505), has no Safaitic reference to knn, King thoughtlessly claims that her erroneous "knn" "is previously unattested in Safaitic." The name exists in Ox 25.

KiWJ 9 (pp. 314B-315A and pl. 51A). - lṣrmn By Ṣarmân. - King misread the initial ṣ and eliminated the 2nd letter; cf. above, pp. 153-154. - Cf. ṣrm of Hu 313.

KiWJ 10 (pp. 314A, 315 and pl. 51B). - wd²b ḥṯṯ Wad²ab has engraved [this].
As in the edition where "bṯṯ" is a printing error, as proved by the commentary, King erroneously puts her "it" between parentheses instead of between brackets. Pikally, the name wd² of WiAM 60 makes it possible to consider wd²b as a theophoric name with -b = ²b, as -l = -²l = ²il god.

B - The graffiti of Wâdî Matah; siglum KiWM.

My stand against the writer's reading hypotheses was already evident in my Miscellanées ..., XVI (1988), pp. 218-221 at the occasion of the review of W.J. Jobling's papers (cf. above, pp. 151-152).

"The 20 inscriptions read below, come from the ḫamāda [sic!] desert southeast of Ma'ān" (p. 37),

the coordinates of which are "30° 12' N - 35° 44' E" (cf. Jordan-Gazetteer, Washington, 1971, p. 231A), in the Wâdî Matah with the coordinates of "30° 11' N - 36° 15' E" (cf. l.c., p. 247B). These texts were published in a paper mentioned above (p. 150) which, to all intents and purposes, is a joint script by King, Knauf and Macdonald. Indeed, she thanks M. Macdonald also "for all the help and advice he has given me" (cf. fn 7 of p. 37) and "Dr. Knauf" "for other useful comments on this paper" (cf. fn 23 of p. 53). Such flatteries always secure very helpful recommendations. As the preceding paper, this one must also be redone all over again, and the photos are of poor quality and difficult to read so much so that no definite treatment of the material can be achieved without a new set of decent photos. As in the preceding paper (cf. above, p. 150), | is given here also the double value of l in KiWM 3, 5, 10 and 15, and
š in KiWM 1, 2, 11, 13 (twice) and 17 (twice).

How convenient and quite nonsensical to maintain 2 values for the same sign! Even in KiWM 5, King dutifully notes (cf. fn 13 of p. 39) that, according to her fugleman Macdonal, "one should expect a squiggly š." How completely insane for the pontificating Macdonald to decree that such a form of letter "should" be "expected"? Is it possible? But, why was the same remark not made in KiWM 3: "lw²l" (p. 39)? The reason is very simple: such a reading would have given the inexplicable "w²š" in KiWM 3 and "dd²š" in KiWM 5 which are unacceptable to them!

King invents twice the inexistent letter "n" of bn son of in order to render her reading intelligible; the letter is not even put between parentheses in KiWM 2, altough it is between brackets in KiWM 7. Finally, a characteristic of the paper is the rarity of textual references to be found in the commentaries. Instead, the reader is given a very alluring pacifier, e.g., "well attested" (p. 39) or most frequently "in Safaitic" (p. 39, etc; more than 20 times); cf. already above, p. 155, the end of the commentary on KiWJ 4.

The general critics of King's method and production already pointed out above (pp. 150-152) and especially her partisanship and her parody of decipherment remain valid here; e.g., here follows the list of 10 (ten) of my publications that King was not honest enough to utilize in her paper - with Macdonald's approval, for sure -, viz. Minaean Inscriptions Published as Libyanite (1968), Miscellanées ..., II (1971), V (1974), VI (1974), XIII (1983), XIV (1986), XV (1987) and XVI (1988), Thamudic Studies (1967) and Safaitic Notes (1970).

I - The most important inscriptions.

The most important feature of this Wâdî Matah Thamudic collection deals with the 2 pericopes whḍḍḏyt of KiWM 2, 4, 7, 13 and 17 on 2 boulders, and
wlhḏyt of KiWM 15 one the 2nd boulder
with the common element ḏyt.
King reads respectively "w ḥḍḍ ḏyt And he made a sheepfold" (pp. 38, 39 and 51) and
"w lh ḏyt And for him is a sheepfold" (p. 51).

In accordance with her characteristic narrow-mindedness, King posits that
(1) "I am unable to explain the [1st] expression if we maintain [as in her translitteration] this interpretation of the letters. Neither the root ḥḍḍ nor ḏyt exist in Classical Arabic" (p. 38) and
(2) "And no other division of the letters would yield any sense" (fn 10 of p. 38).
These 2 statements manifest her stupid infatuation, her narrow-minded childishness and her incapability of doing the job that she, herself, sought from M. Macdonald (fn 7 of p. 37); e.g., how irrational it is to expect to find "ḏyt" as such in Arabic as if there was a perfect identity of derivations in Thamudic and Arabic since -t is tā² marbūṭat! Therefore, as fully expected, she chose the concoctions suggested by Winnett, Macdonald, Knauf, etc, that are most beneficial to her and her future. As far as she is concerned, the basic reason remains the same all through her paper - the simplemindedness viz the Safaitic names are DE FACTO THE PROOF of her reading of Thamudic, as formulated in: "Again in this inscription we should treat the grapheme ḏ as etymologically /g/. The name ngyt appears in Safaitic" (p. 51, in KiWM 14)
instead of her ntyt of pp. 51 and 54B. Such a reasoning is senselessly stupid and gratuitous; for ntyt, cf. Arabic naṭā "palam fecit, divulgavit nuntium" (cf. G.W. Freytag, IV, p. 240A) and the name ²ntyl ²Anṭayil in HaLi 64.

The very 1st question to be solved is: who built what is called ḏyt in the texts? After restoring the genealogical tree of KiWM 13, 15 and 17, King adds that
"the sheepfold is for the older man [in KiWM 15] whilst his son [in KiWM 17] and grandson [in KiWM 13] record having done all the work!" (fn 15 of p. 51)!

What a pity that King did not share with the reader the so-called freak hidden behind

her "!" At any rate her conclusion is erroneous because the wḥddḍyt pericope is also found in KiWM 2, 4 and 7; therefore, those 3 men have also "done all the work" according to King herself who, in addition, takes the author of KiWM 16 as yet the 6th builder of the same ḍyt. The preceding conclusion that at least five (5) persons who, according to King's translation, "made a sheepfold" which belongs to yet another 6th man and with a 7th man using the verb bny built proves that King's rendering of the pericope cannot be accurate. That pericope sounds very much like a temporal participial phrase or clause stating a situation which became a landmark in local history, and as such it is asyndetically apposed to the main clause. The pericope may be read as follows:

wḥ dḍ ḍyt Waḥḥ being the manager of [this] meeting place.

wḥ, ClAS 776. - dḍ, cf. Arabic daḍḍa "diligentissime ministravit"(cf. G.W. Freytag, II, p. 31B). - ḍyt, cf. Arabic ḍawá (i) "se recepit, confugit ad aliquem, noctu venit" (cf. G.W. Freytag, III, p. 33A); ḍyt may thus be a place where people retire, retreat especially for the night; thus, some kind of shelter. Note King's statement (p. 38):

"I have taken tyt to be Arabic tiyyah 'the lodging place or nightly resting place, of sheep or goats' (Lane I p. 366 ; where the Arabic word is not to be found);

and she translates the word as "sheepfold." This is another plagiarism of F.V. Winnett-G.L. Harding, Inscriptions from Fifty Safaitic Cairns, Toronto, 1978 [cf. my review in Orientalia, 48 (1979), pp. 478-528], p. 632: "tyt 'sheepfold' (ar tāyah, tīyah)." Finally, the location of WiWM 10 renders superfluous the engraving of an article or relative pronoun which is understood; cf. already above, this page, viz my restoration of "[this]. Similarly, the location of WiWM 2, 4, 7, 13, 15 and 17 makes equally certain that ḍyt is very well determined in spite of the absence of the article. King's constant translation of ḍyt as "a sheepfold"(the italics are mine) is quite erroneous.

Here follows the reedition of the 7 texts mentioned above.

KiWM 2 (p. 38 and photo in pl. 4a of p. 44, top).

llkm byr wḥ dḍ ḍyt

By Lakîm. He was assayed [here], Waḥḥ being the manager of [this] meeting place.

lkm, cf. lkml of Hu 307/2, and lkmlh of JoDD 5. - byr, cf. JaT 124b/2; here, in the passive voice. - In her statement of the text, King invents the letter n after lkm b to give a meaning to her decipherment (cf. already above, p. 156), but even without as much as a note in her long commentary of 11 lines; indeed, King is well in the line of van den Branden, Winnett and their modern admirers. She further reads ⊥ , clear in her facsimile (p. 43), as "ẓ" in sheepish accordance with F.V. Winnett; cf. my Miscellanées..., XIV (1985), pp. 34-35. Cf. below, after KiWM 17, a further note, pp. 160-161.

KiWM 4 (p. 39 and photo in pl. 4b of p. 44, bottom).

ʾd dl tʾm bn ʿqrb wḥ dḍ ḍyt

ʾAdd has guided Taʾim, son of ʿAqrab [to here], Waḥḥ being the manager of [this] meeting place.

For the beginning of the text, cf. below, p. 163, KiWM 5. - ʾd, cf. JsaT 177. - dl, cf. JaMAR, V (1974), p. 131B. - tʾm and ʿqrb, as in the edition; tʾm, e.g., Hu 408; ʿqrb, e.g., But 483; for the genealogy of these 2 men, cf. below, p. 164 on KiWM 8.

KiWM 7 (p. 39 and photo in pl. 5a of p. 45, top).

///bʾ srk wḥ dḍ ḍyt

///Has returned Sarik, Waḥḥ being the manager of [this] meeting place.

bʾ, e.g., Hu 143. - srk, e.g., Safaitic CIS 1206. - Here again, King is forced to invent an inexistent n to give some meaning to her reading (cf. already above, p. 156). This time, however, she puts it between brackets, "[n]."

KiWM 13 (p. 51 and photo in pl. 8b of p. 48, bottom).

lʾklbn bn bḥll wḥ dḍ ḍyt

By ʾAklabân, son of Baḥlal, Waḥḥ being the manager of [this] meeting place.

ʾklbn, cf. ʾklb in Safaitic WH 176. King claims that ʾklb is "well attested." Yet, G.L. Harding, An Index ..., Toronto, 1972 [cf. my review in my Miscellanées ..., II (1971), pp. 93-150], p. 62, lists one reference only,"WH 176." This example clearly suggests that King does not really understand the words she uses. Cf. also klbn in Thamudic JsaT 624 and Safaitic CIS 4890. - bḥll, a gtll derivation with the reduplication of the 3rd radical; cf. bḥl in Liḥyanite JaL 129a/2, and Safaitic WH 3914. King erroneously reads the 2nd name as "bḥs̈s̈" (transliteration) = "bḥs̈s̈"(translation). By the way, the reader should know that all Thamudic name is italicized only in her transliterations; they are not italicized in her translations and/or commentaries!?!? King speaks of "ḥs̈s̈" in her commentary, thus parsing her "bḥs̈s̈" as b + ḥs̈s̈. Unfortunately, she does not explain what she is doing with the initial "b." Whatever it may be, her "bḥs̈s̈" would be another gtll derivation as the correct bḥll. - Genealogy: qs → ʿbb (15) → bḥll (17) → ʾklbn (13).

KiWM 15 (p. 51 and photo in pl. 6 of p. 46).

lʿbb bn qs wlh ḍyt

By ʿAbbab, son of Qays, and to him [belongs this] meeting place.

Note that the lâm auctoris has no hook at all, not even in King's facsimile of p. 47; same reading as in the edition. - ʿbb, e.g., Hu 472/2. - qs, e.g., But 774/1.

KiWM 16 (p. 51 and photo in pl. 9a of p. 49, top).

lʾmyt bn khln wbny

By ʾAmyat, son of Kahilân, and he built [this.]

ʾmyt, Hu 800/1. King reads the name as ʾnmyt." But the dot which she mistook for the letter n is a flaw in the stone and cannot possibly be a letter because it is located

against the center of the straight bar of the preceding ʾ, and the letter n always is well isolated from the neighboring letters. Note also the outrageous distortion of m in King's facsimile (p. 47): the double line tracing of the letter is filled up to form a thick line about 4 times as wide as the tracing of any other letter; compare my tracing from pl. 49, top with King's facsimile of p. 47: ⎫ and ⎭ where the outline and the proportion height-width are not even respected. It should also be pointed out that pl. 9a is printed on its left side compared with pl. 6 of p. 46. - khln, as in the edition.

KiWM 17 (p. 51 and photo in pl. 9a of p. 49, top).

lbḥll bn ʿbb wḥ dḏ ḏyt

By Baḥlal, son of ʿAbbab, Waḥḥ being the manager of [this] meeting place.

bḥll, WiWM 13. - ʿbb, as in the edition.

Additional note on KiWM 2 (cf. above, p. 158): remarks on fn 11 of p. 38.

LP 341: whdtʿ hsfr is read by E. Littmann as "w(ḫ)dt hsfr and he made this new inscription" (cf. Safaitic Inscriptions, Leyden, 1943, p. 86). King simply mentions "a very doubtful occurrence of ḫdt in LP 341" without the slightest remark of any kind. King refers to Littmann's statement because it is in the line of her note on the discrepancy of the verbal forms in Thamudic and Arabic; but that is not new. Whether Littmann is right or wrong is none of King's concern. Unfortunately, Littmann's reading is unacceptable. The 4 following remarks are in order.

(1) The pericope is separated from the preceding by the same large space as that seen between the preceding pericope and that which precedes. This remark eliminates the possibility of ʾbh his father being followed by a personal name.

(2) "The dot which may be a ʿ in ʿqrb is here probably a flaw in the stone," states Littmann. But it could as well be the letter n.

(3) Littmann corrects ḫ to "ḫ" but the form of the letter would normally suggest completing it as d, certainly not as "ḫ" in which case the aslant appendix would be in the wrong place since ḫ is composed of 3 equal, converging strokes. Thus, Littmann's "ḫdt" is unacceptable.

(4) The reader is faced with a voluntary distortion of the meaning of the words. Indeed, Littmann lists "ḫdt ... he made new(?)" (p. 314B). But to "make new" an inscription is difficult to comprehend. That is the reason why Littmann transferred "new" to the "text" instead of to the verb. I suggest reading the pericope as follows:

whd(y)n hsfr And he dr(e)w this inscription.

hd(y)n, cf. Arabic ḥâna (o), 2nd form "to record, compose, draw" (cf. E.W.Lane, p.938B).

The pericope of the 2nd 3rd of WH 2161 reads as follows:

"wrʿy hmʿzy ḫdt bksʾ ʿqbt He pastured the goats recently on the track to ʿQBT" (cf. F.V. Winnett - G.L. Harding, Inscriptions from ..., p. 333 and facsimile in pl.42 of p. 700). King sheepishly reproduces the preceding quote, underlining that "Winnett reads ḫdt as an adverb." Here again, no remark of any kind is made. Three remarks are in order.

(1) The word ḫdt manifestly is an adjective qualifying mʿzy; cf. Arabic ḥadaṯ "young applied to a horse or an ass or the like" (cf. E.W. Lane, p. 528C).

(2) CIS (p. 78, on no. 523) gratuitously refers ksʾ to the Arabic plural word ʾaksâʾ but still translates ksʾ as a singular, "semita." Cf. Arabic kusʾ "piste" (cf. E.W.Lane, p. 2608C).

(3) The place name ʿqbt is very well-known. Nonetheless, Winnett's facsimile shows "ʿqtb," the last 2 letters are senestrograde although the rest of the line is dextrograde! The pericope may be translated as follows:

And he pastured the young goats along the piste of ʿAqbat.

II - The remaining texts of the Wâdî Matah collection.

KiWM 1 (p. 38 and photo in pl. 2 of pl. 42).

The characteristic of this text is the 2 words wtm and ʾngrt (instead of "ʾnḏrt" of King) to which she applied her crazy argumentation (cf. above, pp.150-151 and 156-158).

The verb ʾtm is translated as "he grieved" as a sheepish endorsement of G.L. Harding-E. Littmann, Some Thamudic ..., p. 46 in HaLi 494, a theory rejected in my Miscellanées ..., V (1974), pp. 100-101: wtm means to perform the breaking act. Note that King is not honest enough to list the preceding publication, like many others (cf. above, p. 157), in her "Abbreviations" (p. 53). Cf. also below, pp. 168-169.

On her erroneous "ʾnḏrt" instead of ʾngrt, King writes that "it would in this context be equally plausible to read g;" but she miserably fails to justify her choice. Furthermore, she advises the reader to "see HIn p. 79 for the name ʾnḏr." Here again, King does what she does best, viz she sheepishly follows G.L. Harding who refers to "2" Thamudic texts, viz "Hu 58,8; 59,4 doubtful" which are HU 61 and 70/1. But, there is only one text: Hu 61 = 70/1, and ʾnḏr is a verb, not a proper name (cf. my Miscellanées ..., V [1974], p. 49).

Instead of her "ʾrs̄" (cf. above, p. 156), on which she does not give any comment, not even a simple mention in her commentary, the name reads ʾrl ʾArwal, an ʾqtl derivation from the root rwl; e.g., JaT 106v and 115b (cf. my Miscellanées ..., XVI [1988], pp. 208 and 237) where the above spelling must be reported as, e.g., in Liḫyanite ʾrl of JaL 166b and 169p (cf. my Miscellanées ..., VI [1974], pp.23 and 26); cf. also pp. 10 and 12 for Liḫyanite ʾrltʿ and bnʾrl of JaL 145i and 148b, respectively. The

names hn²t and km are given the same treatment as "²rš;" viz they did not deserve in King's eye even a mere mention in her commentary; cf., respectively, HaLi 211 and Bu 28.

King is not even capable of giving a complete, accurate description of the 3 horizontal sets of 7 vertical/aslant strokes. The 1st set beginning below the 2nd letter of the text is composed of thin strokes, even thinner than those of the 1st 3 letters, and the 7 strokes are united together by a long horizontal bar supporting the bottom of each vertical stroke. The 2nd set above lh bn km is made of thicker strokes; the central stoke is as heavy as l, and the remaining 6 average the width of the letter m. Finally, the 3rd set located almost in the middle of the loop formed by the last 17 letters is composed of very thick strokes almost as thick as the encircling letters.

KiWM 3 (p. 39 and photo in pl. 4b of p. 44, bottom).

Cf. above, my remark in p. 156. Furthermore, King did not register the last letter n which does not appear in her facsimile of p. 43. The text may be read as follows:

lw²ln By Wâ²ilân. - Cf. JsaT 707.

KiWM 5 (p. 39 and photo in pl. 4b of p. 44, bottom).

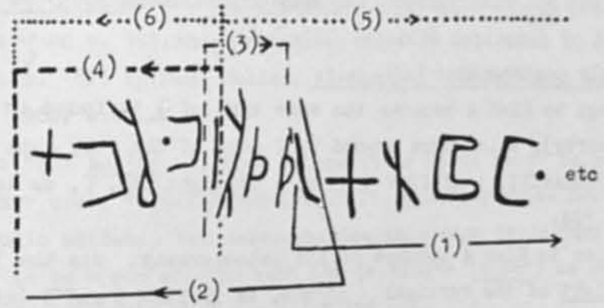
King erroneously considers a unique l to introduce 2 different texts running in opposite directions - who has ever heard of such a scheme? - KiWM 4 and 5 are dextrograde and senestrograde, respectively. King is so fascinated by her blinding thesis on ḏ, ṯ, ẓ, etc, that she is not even aware of, or refuse to acknowledge, the constant fact of the hook of l always pointing in the direction of the text. However, the tiny hook of l of KiWM 14 as drawn in her facsimile of p. 47 is a flaw of the stone (cf. the photo in pl. 8b of p. 48, bottom).

King reads the 2nd and last name of KiWM 5 as "²b(š)" (p. 39) and her commentary notes that "the last letter of the second name poses a problem as it looks more like a f. I think, however, it is possible that the straight part of the squiggle is what was intended as the letter and the rest had been added on." How convincing is the "I think"! And who is the villain responsible for those unspecified additions? And what is the name of the hoepoe or jinn who whispered such an irrational fancy to her ear? Alas for her, her facsimile of the letter (p. 43) shows four "straight" sections, 2 vertical (which one was chosen by her?) and 2 aslant, all making an almost vertical zigzag. But, King's zigzag is not to be seen in the photo of pl. 4b of p. 44, bottom, where the letter is ṯ, a clear cross from which hangs a design in the rock having the form of ∫ (cf. above, p. 152). Furthermore, the bottoms of the lower fork of her "²" are joined together by a rounded curve; thus, ẓ, and the last name reads ẓbt mishap; cf. the noun ẓbt mishap in Safaitic CIS 4568 (cf. my Safaitic Notes, Washington, 1970, p. 71, note 95). Finally, for King's origin of "squiggle," cf. above, p. 156, Macdohald's "squiggly."

Still King remains unsatisfied: her unique l introducing 2 texts is like a piece of yeast sticking in her throat; and her mind is toying with l of dd²l; finally, she got a genial idea expressed at the beginning of her commentary on KiKM 5 as follows:

> "The inscription uses the lam auctoris of no. 4 and reads from right to left. It is equally possible that they should be taken as two texts reading in opposite directions and both using the l of dd²l as the lam auctoris. They would read l²dd and l bn ²b(š)."

King's ineptitude and ignorance are so crass that the reader can only gape at it and wonder. My following remarks are better illustrated below by my tracing (from the photo of pl. IVb, p. 44) of KiWM 4 (beginning) and 5, showing King's successive dissections of the line compared with my interpretation based on palaeography.



Caption. - The arrows indicate the direction of the reading. - For King's reading, cf., l.c., p. 39, ll. 4-16.

solid line: (1) King's no. 4: "l t²m bn ...;" dextrograde letters; (2) King's no. 5: "l dd²l bn ²b(š);" 4 dextrograde and 3 sinistrograde letters;

hyphened line: (3) King's 2nd reading: no. 5a: "l²dd;" dextrograde letters; (4) King's 2nd reading: no. 5b: "l bn ²b(š):" sinistrograde letters;

dotted line: (5) my reading of KiWM 4: ²d dl t²m bn ... (cf. above, p. 159); dextrograde letters); and (6) my reading of KiWM 5: lbn ẓbt; sinistrograde letters.

A few remarks of commentary are in order.

1 - There is no plural in the initial phrase of the above quotation, to which the "they" could be related ... and only the context suggests that the plural "they" refers to the 10 letters of the text.

2 - No reason - as usual - is given to justify her "equally possible"... and that is precisely the problem with King: it suffices that her wild haughtiness imagines some thing and it automatically becomes at least a self-established possibility. Summarily, this text, KiWM 5, will remain a typical example of King's haughty ignorance because she herself created twice the same problem, and twice she suggested the same solution, viz

(follows p. 163A)

an inscription started by the lâm auctoris and her theory of 2 l's introducting each 2 texts. Although relishing, like Macdonald, to pontificate her oracle-like gratuitous statements, she does not even know what is obvious in Thamudic: an inscription needs not be introduced by l- or w-, a verb (e.g., wd[d]), etc and, to make it worst, G.L. Harding - E. Littmann's volume, which she literally plundered (cf. above, pp. 152-153), contains many examples of such a phrasaeology. The preceding case exemplifies King's working method for using what she wants while discarding what she dislikes and/or does not understand although her knowledge of North-Arabic is less than embryonic.

3 - Propelled by her ignorance to find a meaning to the complex left of t²m, she connects the l to the left with the 9 following letters on the left. Thus, the 1st 4 letters of her no. 5 are all dextrograde, and they are followed by 3 sinistrograde - How incredible! - and by 3 neutral letters.

ldd² are manifestly dextrograde:

a - l, ⌊, belongs to KiWM 4 because the same type of l is found in KiKM 6 and 8 (engraved nearby), which were traced by 2 sons of the very same t²m, viz gsr and hnnt; CONTRARILY, l of KiWM 5 is a straight bar, |, as immediately to the left of ²dd.

b - ² belongs also to KiWM 4 because of its palaeography, viz the lower appendix is on the right of the vertical stroke, as in KiWM 6 and 8 (cf. above), viz ; in her KiWM 5, on the contrary, that lower appendix is on the left side, .

c - The 2 d's are manifestly dextrograde; they simply cannot be whimsically attributed to a sinistrograde inscription.

4 - The amazing result of King's 2nd reading is that her 2nd study (?) eliminates from her n.5 one etter, the l on the left of t²m. Naturally, King does not even mention the elimination of the initial letter of her no.5; King is so blinded by her thesis that she did not even realize that her new verdict actually beheaded her official reading of no. 5 given only 3 lines above!!!

5 - King crowns her useless commentary with this remark:

"The name ²bš occurs in CSNS 385 but bn ²bš, if that is how it is to be read, would be new."

The following remarks are in order.

a - It is unfortunate that, in a case like this, King fails to remind the reader that "CSNS" - a siglum that she copied from her trainer Macdonald - introduces a Safaitic (not Thamudic) text; such an illogical attitude is well in the line of her fundamental, preconceived, insane theory (cf. already above, p. 157).

b - V.A. Clark claims that the etymology of ²bš could "possibly" come from Arabic bašša.

(follows p. 163B)

this etymology in unacceptable because it would demand an unjustifiable dissection of the name ²bš since the root ²bš does exist in Arabic; cf. ²abaša "to collect (a thing)." Apparently Clark refers his suggestion to the mention of the Arabic name "Baššā" from W. Caskel, Ǧumharat an-Nasab, Leiden, 1966, 225/1," which must be corrected to "225/2." Of course, such a name is not a justification at all. Clark's argumentation is further doomed by the 2 following Arabic names, viz "Bicha بيشة" and "Boucha بوشة" (cf. Vocabulaire destiné à fixer la prononciation en français des noms des indigènes, Alger, 1891, pp. 51B and 57B, respectively) from the roots bš and bš; cf. also the personal name بيش in M.J. de Goeje, Annales quos scripsit ... at-Tabari, Leiden, Indices, 1901, p. 83; the geographical names بَيْش, بَيّش, بِيش and بيشة in F. Wüstenfeld, Jacut's geographisches Wörterbuch, Bd. VI, Register, Leipzig, 1870, p. 45A-B; and also بَيّش, بَيّشة and بيشة on p. 18A, and بُوشان Büsân on p.16B (cf. also L. Forrer, Südarabien, Nendeln, 1966, p. 182) in D.H. Müller, al-Hamdânî's Geographie der arabischen Halbinsel, Leiden, 1968; etc.

c - King phrase "but bn ²bš, if that is how it is to be read" is nothing but the useless repetition of her crazy "equally possible." It, however, shows how ignorant and fatuous she is in Thamudic matters: her narrow-minded knowledge is limited to a few well-known rehashes, such as bn = son of. The verb lbn (= King's "l bn") is well-known in Thamudic as "to be struck with violence;" e.g., JaT 118 c/2.

KiWM 5 may be read as follows: ← lbn ṣbt Ṣawbat was violently struck [here].

For ṣbt, cf. above, p.163.

Summarily, the reader may in all objectivity apply here to the "School of African and Oriental Research" of London what he wrote above (p. 90), viz King "peut jouer avec" The School of African and Oriental Research with the help of M.C.A. Macdonald "et lui faire croire que la girouette, symbole du mouvement perpétuel, reste le meilleur expédient pour éviter une difficulté! Pour sa part, le lecteur ne se prive pas de faire comprendre à" G. King et à "The School of African and Oriental Research" "que le pauvre en esprit n'est pas celui qu'[ils] pense[nt]."

KiWM 6 (p. 39 and photo in pl. 4b of p. 44, bottom).

lqsr bn t²m By Qâsir, son of Tâ²im.

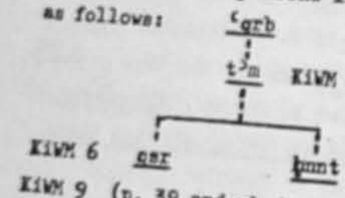
Same decipherment as in the edition which does not give a single word of commentary on

either name. - gsr, e.g., Rees 145, and gsrlt in Lihyanite JsaL 231. This writer is a son of the author of KiWM 4; for the genealogy of these 2 men, cf. below, the commentary on KiWM 8; cf. already the note on p. 163A.

KiWM 8 (p. 39 and photo in pl. 4b of p. 44, bottom).

lḫnnt bn t²m By Ḫannat, son of Tā²im.

As in the edition, which does not have any commentary at all. - ḫnnt, Ḫā²il 58. - The genealogy of the persons listed in WiWM 4, 6 and 8 (cf. pp. 53B-54A) may be presented as follows:



For some remarks on King's "Index" (pp. 53-54), cf. below., p. 174; for ʿgrb and gsr, cf. p. 53B, and ḫnnt and t²m, cf. p. 54A.

KiWM 9 (p. 39 and photo in pl. 5b of p. 45, center and bottom left).

l²sd bn tm bn ʿbd By ²Asad, son of Tamm, son of ʿAbd.

As in the edition, which does not provide any commentary of any kind. - ²sd, e.g., Hu 282. - tm, e.g., Hu 642/1. - ʿbd, e.g., JaT 126a/3.

KiWM 10 (p. 39 and no photo).

King states that "faint traces of letters" are located "between the end of no. 3 and the beginning of no. 2."

Nothing is visible in pl. 2 of p. 42; and the facsimiles of pl. 3 of p. 43 indicate only the approximate location of text no. "10." That very location is enlarged in the photos of pl. 4a and 4b of p. 44, although King does not refer to those photos, and the captions added to them do not mention no. 10 either. King, however, refers to pl. 5b of p. 45, bottom right, which contains only a facsimile composed of 5 letters. She also adds that "there is a t at right angles to the letters which is probably a continuation of the text."

But, even her facsimile does not show any "t" of which, in any event, she does not indicate the exact location. Such a miserable documentation is manifestly insufficient and too sketchy, and the facsimile cannot be checked. The whole number is unacceptable. And what can the reader think when he learns that King was granted a Ph.D. thanks to such a working method - apparently - with the full support A.F.L. Beeston and M.C.A. Macdonald?

KiWM 11 (p. 39 and photo in pl. 5a of p. 45, top).

King's facsimile in pl. 3 of p. 43 is not supported by the grayish photo. Nothing can be done with the present documentation: another casualty of King's carelessness and ineptitude sheltered by M.C.A. Macdonald's "je-m'en-foutisme de tous les jours."

KiWM 12 (p. 51 and photo in pl. 8a of p. 48, top, which also carries King's facsimile between the caption and the scale).

King's haughtiness is self-evident when she claims that "perhaps the ² is the inscriber's error for y"!!!

How a foreign conceit puppy, a mere nursling dares to accuse a Thamudaean grown-up of such an "error"? Such haughty an attitude may be traced to her trainer because the same frame of mind is clearly professed as an obvious, matter-of-fact orientation by M.C.A. Macdonald (cf. PSAS, 16 [1986], p. 101): "It is possible, of course, that it is a mistaken repetition of f" (italics are mine),

an attitude promptly denounced in my Miscellanées ..., XV (1987), p. 197:

> "What a pity that the engraver did not have Macdonald to teach him his own native language and his stone carving!" This remark also applies to baby King.

Furthermore, the upper tip of letters nos. 1 and 6 (starting from the end) is cut off by the photo; it is thus impossible to know the value of those 2 letters. Moreover, at least 5 letters of King's facsimile cannot be checked on the photo. This text is another casualty of King's carelessness. As already noted above (p. 160) in the commentary on KiWM 16, pl. 6 of p. 46 (and also pl. 7 of p. 47) is placed on its right side compared with pl. 9a of p. 49, top.

On her name "ʿbdtd" which she distors to "ʿbdgd," King mentions that "there is a doubtful reading in Safaitic (C 1127)." She is so pleased and satisfied with her Olympian oracle that it says it all and is perfectly satisfying. Her reference was copied from G.L. Harding, An Index ..., p. 398, which, along with O. Eissfelt in ZDMG, 104 (1954), p. 103, represents the 3rd stage of the saga of this name, viz for these 2 authors, the name is absolutely certain. The 2nd stage is shown by CIS (p. 150A) according to which the last letter only is doubtful, viz "ʿbdgd." But the 1st stage is that of the publisher himself, E. Littmann, "ʿ[b]d-g[d]" (cf. Zur Entzifferung der Ṣafâ-Inschriften, Lepizig, 1901, p. 50). In all probability, King follows CIS. But even CIS's transcription should have been enough for King to investigate and find out the reason of that circlet. And Littmann's reading of his "[b]" joins together 2 letters widely separated from each other, and the letter he restores is ḏ, and certainly not "b" or [b]." In fact, Littmann's reading is also wrong as far as the last word is concerned, "frs," because the 1st letter is š. Finally, the empty space separating the 1st 3 letters of the copy from the rest is about twice as wide as any ordinary space between the letters; these 3 letters may safely be considered as a poor beginning of the text, which may be read as follows: ldmm bn nšbt ḥšrs To Damîm, son of Našbat [belongs] this nasty camel. For dmm, cf. Ar damîm "smeared" (cf. E.W. Lane, p. 911A). - šrs, cf. Arabic šarisat "A she-camel evil in disposition" (cf. l.c., p. 1532B).

KiWM 14 (p. 51 and photo in pl. 8b of p. 48, bottom).

lntyt bn ḥmly By Natyat, son of Ḥamlay.

Same deciphering of the letters as in the edition; cf. however, my remarks above, p.157, on the 1st name, and p. 162 on the so-called hooked l. - hmly: King does not comment on the name; cf. Safaitic, e.g., CIS 1259, and Thamudic hml, feminine in Hu 565, and halt in WiAM 42.

KiWM 18 (p. 51 and photo in pl. 10a of p. 50, top).

lwhḏ bn mʿrh bn mty bn nʿrt

By Wâhiḏ, son of Maʿiruh, son of Matay, son of Naʿrat.

The photo is particularly dark: it is difficult to see the exact form of all letters, and King's facsimile in pl. 10b of p. 50, bottom, upside down compared with the photo itself, is erroneously labeled by the encircled number as "19." - whḏ, cf. Arabic waha-ša "Traxit humi" (cf. G.W. Freytag, IV, p. 444B). - mʿrh, cf. Arabic maʿira "to become poor" (cf. E.W. Lane, p. 2723C). King reads "mʿnlh" considering as a n, which does not exist in her facsimile, a flaw in the stone comparable with that after whḏ which she simply discarded, and also reading l a letter shown as r in her facsimile. - mty, e.g., HaLi 10 = 266 = 298. - nʿrt, e.g. Safaitic JaS 131. King's commentary explains that the 2 consecutive n's of bn nʿrt "are doubtful as in some lights they look as thought they are joined to form one letter, possibly an ʿ."

The difference of shades of gray makes sure that the 2 n's are quite distinct from each other. Besides, a "ʿ" would yield an incomprehensible text, bn mtyrʿʿʿrt.

KiWM 19 (p. 52 and photo in pl. 9b of p. 49, upper center).

This very short graffito exemplifies another aspect of King's method.

(1) She reads the text as "l m(ʿ)b" which becomes "By mʿb." Thus, the simple transition from the reading to the translation is in itself an argument good enough to eliminate all traces of doubt in the reading of the letter. King's hoepoe is quite a magician! But because of an unknown reason, the doubt resurfaces in p. 54B, but it now affects the whole name, "mʿb ?" And such a crazy method got her a Ph.D. diploma! Cf. above, p. 164.

(2) Her whole commentary on the text reads as follows:

"I am not sure how to read the second letter of the name but possibly it is joined at the bottom forming an ʿ."

The "possibly" becomes a certainty in her facsimile (reproduced to the right of the photo) where a short aslant stroke, inexistent in the photo, joins together the lower extremities of the 2 long bars of the letter and, therefore, closes the geometrical design! Frankly, her hoepoe is much more than a magician! This is the indisputable proof that King makes her facsimiles according to her preconceived theories; unfortunately she seems unable to adjust her transcription and commentary to her dream. The letter is s.

lmsb By Misabb. - Cf. Ḥâʾil 195.

KiWM 20 (p. 52 and photo in pl. 9b of pl. 49, bottom).

King reads "l ml --." Such a reading is unacceptable: her "m" belongs to 2 letters, and there is no sign after the 4th letter. The photo, however, is much too dark to allow the definitive study of any letter, and her facsimile to the right of the photo is useless.

The main conclusion of the preceding study is twofold, viz not a single text is properly published, and the documentation of 5 (five) texts out of 20 (thus, 1/4 of the material), KiWM 10, 11, 12, 19 and 20, is quite unsatisfactory and unacceptable; these texts are discarded until proper documentation is available.

III - King's additional list of references with # = ḏ (pp. 52-53).

On the basis of her erroneous treatment of the main pericope of the Matah collection (cf. above, pp. 157-159), King claims that

"there is a certain amount of evidence to support the suggestion"

that her thesis is correct, viz # = t and not ḏ. But what is the meaning of an expression, such as "a certain amount of evidence"? In fact, her 50 lines, of which 13 belong to footnotes, are an empty verbiage lacking of any kind of proof. She lists 19 names with 33 texts, 25 of them come from the HaLi collection (cf. above, p. 153 for the title). As already denounced above (p. 157), her basic argument to which she almost gives total priority is the existence of the Safaitic word her preconceived idea wants at all cost to find in Thamudic. It is indeed astonishing that the basic information that those authors take no heed of is the constant value which letters, such as 8 as t, have all through the Arabian peninsula. An outstanding example of their reasoning is the frequent verbal expression wgm ʿl he mourned upon. When the expression wtm ʿl was found in HaLi 494, G.L. Harding immediately posited that "the meaning is quite clear" (cf. G.L. Harding - E. Littmann, l.c., pp. 46 and 56A-B), and E. Littmann sealed the case by stating that "the verb wtm stands here where in Safaitic we have wgm." Of course, those authors could not care less that wtm coresponds to Arabic waṯama to break. Cf. already above, p. 161, and my answer to E.A. Knauf in my Miscellanees ..., XIII (1983), p. 188. Finally, King seems to be willing to strike what indeed looks - at least in her eyes - a rational, viz an appeasing, quietening note; she writes

"only cases where the readings are fairly certain are listed here" (fn 16 of p. 52).

Unfortunately, 2 important questions remain unanswered, viz

(1) what is the meaning of "fairly certain"? and

(2) what is her yardstick?, viz what in her opinion renders a reading "fairly certain"?

The preceding pages contain many examples of her misreadings all of which most certainly were much more than "fairly certain" to her. Summarily, whatever her yardstick really

is, it still is wholly unacceptable, as repeatedly proven in the preceding and following pages.

Entry 1.— "bdd" of JoPRP 16 is King's 1st example (p. 52). Beside copying W.J. Jobling's reading, King states that "bdd is unattested as a name and the root is problematic. bdṯ occurs in Safaitic." What is the meaning of a "problematic" root? What is the "problem"? As usual, King is at her best instilling a Safaitic name as the sole proof of her brainwashing process. Then, she refers to her note 17 which may safely be disregarded as a childish muddle beside the point. The text was republished as JaT 129 in my Miscellanées ..., XVI (1988), p. 200: bnḥ dd Dadd was a meatcutter.

Entry 2.— King mentions "rḍ" of JaT 127 which she claims "occurs in Safaitic" (p. 52). King is not honest enough to refer to the preceding publication ... as usual. She sheepishly endorses W.J. Jobling's reading of "l rḍ bn tm" (cf. ADAJ, 26 [1982], p. 202B, fn 6; in King's p. 54, correct "Pl. 202" to "P. 202." This text was also reedited with a tracing in my Miscellanées ..., l.c., pp. 162-163:

lrḍbn tm By Raḍbân. He was in love excessively.

Here again, King is not honest enough to refer to the preceding publication, and she crowns her dishonest brainwashing by adding that "rṯ and rṯ²l are attested in Safaitic." This addition is a completely irrelevant waste of paper.

Entry 3.— King lists "s̆bḍ" of HaLi "197, 401" with the commentary: "Root unknown. s̆bṯ is unattested as a name but for the root, see Lane ..." (p. 52) with reference to fn 18: "Littmann suggests supplying a ṯ and reading the name s̆bṯ in TIJ 315a. He reads the name in LP 1132 but frṯ in HIn p. 465 is the correct reading." The 1st name of HaLi 197 = 401 is lbḍ Ilbawḍ; cf. bḍ in HuIR 134, the verb bḍ (= bwḍ) to halt of Hu 726, and Safaitic bḍ²l of CIS 5555. — In HaLi 315, King sheepishly follows in E. Littmann's footsteps when dividing the text into 2 independent sections although the palaeography and the relative position of the 2 sections point to a single text only, which may be read as follows:

l²tmg bn khl rl r.. bn ᶜwn

By ²Aṯmag, son of Kahil. Has slavered [here] R.., son of ᶜAwân.

For ²tmg, cf. my Miscellanées ..., XIV (1985), pp. 24 and 328C. - khl and ᶜwn, as in the edition. - rl, cf. Arabic râla (i) "to slaver (child)" (cf. E.W. Lane, p. 1203C). - r..: contrary to Littmann (followed by King), 2 letters must be restored: the empty space is 1 cm wide but it is between 0.3 and 0.5 cm between the other letters. - Finally, her remark on G.L. Harding's "correct reading" of LP 1132 is another proof of her dishonesty: the correction has already been published almost a quarter of a century ago in my Safaitic Notes, Washington, 1970, p. 75, note 107, another publication which her staunch

partisanship prevented her from listing in her p. 54.

Entry 4.— The name "s̆ḍᶜ" of HaLi 22 is commented upon by King as follows:

"Root unknown. The second letter should perhaps read l. lṯᶜ is attested in Safaitic" (p. 52) with reference to fn 19:

"In SIT 31, however, the reading is doubtful," so states King.

(1) The "second" of her quote would be accurate were she speaking of the text itself. Since she speaks of the name only, the word "second" is wrong and must be corrected to "first."

(2) HaLi 22 reads as follows: llḍy bn ²dn By Laḍay, son of ²Adin; cf. my Miscellanées ..., XIII (1983), p. 233 and 237-238, another publication which, like the other volumes, King is not honest enough to list in her p. 54. — For "(3)," cf. below, p. 175.

Entry 5.— The name ḍ̇gm of WiAM T32 (cf. F.V. Winnett in AION, 31 [1971], p. 446 and photo in pl. 2, no. 8; bottom left) is explained by the editor as follows:

"It is noteworthy that in these texts [viz WiAM T32 , HaLi 161 and 193, all 3 repeated by King] the ġ lacks the diacritical stroke which is ordinarily added to distinguish it from k."

King's summary of Winnett's statement shows her distorting the written words, viz

"As Winnett points out the second letter in all these texts looks more like a k."

This is untrue: the letter does NOT "look more like a k" because the letter IS k. Furthermore, Winnett's argumenting method which is illustrated here once more, is nothing but irrational because, were his argument valid, Winnett or any of his devotees could correctly posit that each of the 5 following letters

𐤋 l	"lacks	𐤇 ḥ, 𐤎 s and 𐤑 ẓ;
𐤊 l	the diacritical stroke	𐤊 k,
𐤏 ᶜ	which is ordinarily	𐤅 w,
𐤁 b	added to it to distinguish it from"	𐤈 ṭ, and
𐤊 k		𐤂 ġ,

and therefore, e.g., 𐤋 could be read k, ḥ, s or ẓ besides s̆ (which is done when it suits any of those authors) whenever the choice would make their reading easier. Furthermore, Winnett does not justify his previous statement. But the self-proclaimed all-knowing King knows better: she claims that "the name has been read ḍ̇gm because of the difficulty of the root ḍkm" (p. 52). How ridiculous it is to speak of "the difficulty of the root ḍkm" since there is no such a root: something inexistent cannot be difficult or "problematic" (cf. above, p. 168) or easy since by definition it does not exist. Then to seal her well-exhibited idiotism, King adds in fn 20 that my "explanation of the root is unsatisfactory;" cf. my Miscellanées ..., VI (1974), p. 25. Here again, the statement is manifestly stupid: I do NOT speak of "the root" ḍkm. What I stated is: "ḍk as a verb in Tham (e.g., Negev 1105/2; cf. JaMIL, p. 100, note 53) and the personal name in Saf Ox

45" and, therefore, the nominal derivation qtlm, wich is well-known in Lihyanite, Thamudic and Safaitic, applies here too. If she rejects such a formation, which she does not dare to spell out, her attitude would only confirm her total ignorance as well as her boundless haughtiness, for which she got a Ph.D. diploma! Cf. already above, p. 166.

Entry 6.— King notes on ḏnt of HaLi 352 that

> "the name is all right. tnt is unattested in Safaitic but it occurs in South Arabian."

It is difficult to understand the relevancy of such a comment. Here again, what is the meaning of "all right"? To what does she give her magnanimous approval? Instead of wasting her time on "tnt" which is not found in HaLi 352, she would have been well inspired to refer to the nominal derivations of the root ḏn = ḏwn in both Thamudic and Safaitic.

Entry 7.— The name ʿḏ of HaLi 351 is said to be "well attested in Safaitic" (p. 52). What is the relevancy of such a remark which does not at all affect the reading? King adds that "ʿṯ also occurs in Safaitic." So what? Again, how useless is such a comment which does not affect the reading of the Thamudic name! But she does not even know that ʿṯ is also attested in Thamudic Naveh Negev 1, and is a verb in Hu 407. These 2 sigla make her reference even more worthless.

Entry 8.— On the name ʿḏmn of HaLi 459, King uselessly repeats her performance on ḏnt, viz "The name is all right" (p. 52) whatever that actually means! Then, here also, she adds "ʿtmn is attested in Safaitic." She is so infatuated with Safaitic that she eliminates what is Thamudic, viz the names ʿtm in JsaT 126, and ʿtmn in Hu 427.

Entry 9.— The root ġḏ = ġḍḍ is widely attested in Thamudic with the perfect Arabic parallel ġaḍḍa to be full of vigor, of youth; cf. G.W. Freytag, III, p. 279A and my paper in RiOr, 24 (1967), p. 285B on the verb ġḍ of JsaT 24. King refers to the names ġḏ (e.g., HaLi 5), ġḏʾl (e.g., HaLi 103 = 104, contrary to King's erroneous "103-4," a siglum which she copied from G.L. Harding, An Index, p. 156) and ġḏlh (e.g., HaLi 96). To even mention the root ġṯ because some of its derivations are also attested in Safaitic points to an almost congenital pruritus amounting to madness especially since the root ġḏ is also known in Safaitic. Furthermore, King carefully avoids mentioning not only the verb ġḏ of JsaT 24 (cf. already above) but also the 2 names ġḏh of Hamm 8, mġḏ of HaLi 191 and 260 and the reference Parr 79 to ġḏ although the siglum as well as mġḏ are listed by G.L. Harding under the name ġḏ. It is very strange indeed that 11 of the 12 texts listed by her are HaLi numbers; and King lists separately ġḏʾl, ġḏlh and mġḏ as Entries 10, 11 and 16. Summarily the preceding details prove that King either does not wish to understand the words she uses or is incapable of performing the task she - not someone else - imposed upon herself. Yet, she got a Ph.D. for her insane method (cf. above, pp. 164, etc).

Entry 12.— The name lḏmt of HaLi 6 and 27 is commented by King as follows:

> "No occurrence in Safaitic. ltmt is well attested in Safaitic" (p. 52).

It is quite impossible to treat such a comment as a sign of honesty. Indeed, lḏm exists in Safaitic HaCH 125; and Thamudic ltmt is found in JsaT 128, the verb ltm in Ent 545, and the name ltmdd in Hu 801. Again and again, the presence of a name in Safaitic cannot in any chape or form justify the reading of a Thamudic name, ... of course!

Entry 13.— King brings up again the erroneous name "mrʾykḏ" of KnaufS 2 (p. 53). As already mentioned above (p. 169 in Entry 4), my Miscellanées ..., XIII (1983), is not listed in King's bibliography. Yet KnaufS 2 was republished there in pp. 288 and 240, where it is shown that Knauf's difficulty comes from the narrow-mindedness of his approach to Thamudic and Safaitic texts and his crave for sensationalism or novelty forcing him to all kinds of absurd suggestions which have come to characterize his work and be synonymous of his writing.

In fn 23 of p. 53, which is another proof of the partisanship of the 2 K's (King and Knauf), King thanks "Dr. Knauf" for bringing to her attention the name "tmyġt in Ent. 433 and 663." Such an admission of her limited documentation underlines even more acutely the haughtiness of a mere baby (cf. above, p. 165) venturing into a field with a less than minimal knowledge of it. The 2 references of "Ent. 433 and 663," Knauf copied them from G.L. Harding, An Index, p. 138. In fact, tmyġt Tammyaġut is found in Ent 433/2 (cf. my Miscellanées ..., V [1974], pp. 35-36). "Ent. 663" is known since A. van den Branden's 1950 publication as Hu 480/1 (Hu 480 = Ent 663+664+665), and the complete reading of Hu 480 was published in my Thamudic Studies, Washington, 1967, p. 38. Hu 314 = Ent 403 = 423 (cf. l.c., pp. 47, 52 and 81A) also mentions tmyġt.

Although unnoticed by King - what else could she do - to the present case also belongs her last Entry 19 "ykḏ" of WiAM T6 (p. 53) since it is the 2nd element of her "mrʾykḏ," the present case. Here again, King would have done well to keep her information up-to-date: the text has already been republished in my Miscellanées ..., XIII (1983), pp. 188 and 243.

Entry 14.— King's comment on the name mḏʿn of HaLi 261 is another example of her incompetence: her whole commentary reads "No occurrence in Safaitic" (p. 53). How is it possible to persist so stubbornly in so crass an ignorance: the name comes from ḏʿ = ḏwʿ; Muduʿân. Cf. the name ḏʿ in WH 459 and the verb ḏʿ in the passive voice in CIS 2869. Where were Macdonald and Knauf?; cf. also below, p. 172, "Entry 17."

Entry 15.— King lists mʿḏ of HaLi 300 (p. 53). The saga of this name begins with E. Littmann declaring that "the name mʿḏ is Safaitic" (cf. G.L. Harding - E. Littmann, Some Thamudic ..., p. 32) for the fallacious reason that it was previously

known in Safaitic! Then comes King who even ignoring the existence of the root m^{c}ḏ (cf., e.g., G.W. Freytag , IV, p. 193A) would suggest reading "m^{c}ṯ" simply and exclusively to comply with her crazy thesis which is, of course, anything but a reason. And to make things even worse, she comments saying that that "root is problematic," an adjective used already in her "bḍḍ" of her Entry 1 (cf. above, p. 168). Here as in her "bḍḍ," King is not honest enough to admit that the root does not exist. Obviously King revels in disregarding the obvious and looking for difficulties where there are none. For the 2nd half of the text, cf. already my Miscellanées..., II (1971), pp. 104 and 106, letter "g."

Entry 16. — King mentions mḏṯ of HaLi 191 and 260 (p. 53), on which she practically gives the same comment as on lḏmt of Entry 12, and my answer remains the same (cf. above, p. 171). The root ḏṯ is so well known in Thamudic onomastics (cf. above, Entry 9 in p. 170) that it is insane to attempt to read ġt because of a stupid thesis and all the more so because both ḏṯ and ġt are also well attested in Safaitic onomastics. Therefore, I suppose that King, when dealing with ḏṯ, would correct it to ḏṯ, but when dealing with ġt, she would correct it to ḏṯ. What an anile "Beestonian" weathercock!

Entry 17. — King lists "whḏt" of WiAM T2 and HaLi 469 (p. 53) to find herself in a cul-de-sac according to her commentary which reads as follows:

"Unattested in Safaitic. Both the roots whḏ and wht are problematic."

Here again is the famous adjective "problematic" with the meaning of "nonexistent," and no effort is made to solve the problem. It is almost unthinkable that King would run out of steam or trick to prove her theses, and equally inconceivable that neither Knauf nor Macdonald could provide her with any help when she needed most;cf.above,"Entry 14."

The initial name of HaLi 469 reads whḏt Wahḍat; cf. my Miscellanées ..., II (1971), p. 147. Harding's facsimile (cf. G.L. Harding - E. Littmann, Some Thamudic ...,pl.22) completely distorts the letter seen on the photo of pl. 21 by taking in his letter the upper section of a natural design [and eliminating the upper vertical section of the original ḥ.

The 1st half of WiAM T2 reads as follows:

lw ḥḏt - Has hidden [here] Ḥaḍḍat -

lw = ly to hide, conceal, e.g., in But 698 (cf. mys Miscellanées ..., V [1974], p. 42); cf. also ly as the name of a person (e.g., But 242) and of a place (Moritz 6). - ḥḏt,cf. Safaitic ḥḍ of CIS 3200.

Entry 18. — The 1st name of WiAM T29, wḏm, is listed by King, and her commentary is worth being her last expectoration; it reads as follows:

"Only occurs here and the root is difficult. For the root wtm, see Hava p.850" (p.53).

As she has done with the adjective "problematic," King does not explain here why the

"root is difficult." Where is the difficulty? Then, she fails to refer to Safaitic wḏm of LP 770; cf. my Safaitic Notes, p. 82, note 127. In my Miscellanées ..., VII (1974), p. 88, 1.2 of the commentary on JaL 106c, correct "wḏym" to wḏmn; and 1.1 of the same commentary also notes that the 2nd name of the same text reads ġzm Ḳāsim. Worst of all, King refers wḏm to "the root wtm." But, in wtm of KiWM 1 (cf. above, pp. 161 and 167), she refers to "wgm"! How stupid and loathsome! Cf. above, p. 172, ġt and ḏṯ.

Entry 19. — The name "ykḍ" is dealt with at the end of the study of Entry 13 (cf. above, p. 171).

IV - King's last addition (p. 53).

As an introduction to the final presentation of her thesis (p. 53), King gives some details on an information released in the introduction (p. 37) concerning "an unpublished paper" (fn 4) by F.V. Winnett. While denouncing King's abuse of that paper, I wish to emphasize that it is also an ignominious initiative because all her paper and especially her last addition of p. 53 practically demonstrate F.V. Winnett's thesis to be grossly erroneous ... and he cannot answer! It is further incomprehensible that M.C.A. Macdonald would give his consent to such paper and even contribute to it ...against F. V. Winnett to whom he owes so much, viz without him, he would be nowehere and he would not have at hand F.V. Winnett's documentation sent to him from Canada! This brings to mind the famous proverb: "Faites du bien à un vilain, il vous fera dans la main." The news released by King on F.V. Winnett is disregarded here, but note that no. "389" of HaLi of her text is an error instead of 398, and also that she already dealt partly with HaLi 300 (but no cross-reference!) in Entry 15; cf. above, pp. 171-172, where the reference is given for the 2nd section of the text.

King's last fn 25 is another example of King's worksmanship. She refers to 2 texts "published by W.J. Jobling," showing the letter with 2 concentric circles.

The 1st, of which she says that "I am not sure how to interpret the former of these," was republished as JaT 109 in my Miscellanées ..., XVI (1988), pp.212-213: the letter is ḥ. Of course, no reference to that publication ... because it would be honest to do so!

The 2nd text (cf. ADAJ, 27 [1983], pp. 206-207 and pl. 46) is JoPRF 19 which reads as follows: gb^{c}t bn ntd Gubba^{c}at, son of Natid.

Jobling restores an initial l which he puts between 2 parentheses, but they disappear in the translation, "By." That King cannot restrain herself is proven by the 2 following facts.

(1) She does not object to Jobling's erroneous restoration of an inexistent lâm auctoris because she did the same in KiWM 5 (cf. above,pp. 162-163).

(2) She had to inject her fancy mirage into the present text and she says that "if the [2 concentric circles are] read as ḏ we would get ḏbᶜt which is well attested in Safaitic."

It is as when in bygone days someone would jokingly say "Would my mother wear pants, she would be my father." Of course King is not honest enough to inform the reader that gbᶜ is attested in JsaT 485 (cf. also my Thamudic ..., p. 43). - ntd (instead of Jobling's nty"), cf. Arabic natida "quievit, constitit" (cf. G.W. Freytag, IV, p. 238B).

V - King's word list (pp. 53-54).

In what she incorrectly calls "Index" (p. 53), King is supposed to register all words mentioned in the 20 numbers of her Wâdî Matah collection. If the reader is completely unable to recognize the Arabic abecedary in her list, it is because it was cut into 4 sections which do not follow each other logically in the book, viz the reader must go from p. 53A directly to p. 54A and back to p. 53B followed by p. 54B. Another irregularity of the list is that the last 2 entries of p. 54B must be transferred and added to the last 2 entries of p. 54A as they are the 2nd half of these 2 entries. Furthermore, with a view to making very easy (?!?!) for the reader to find the exact place of any given word, King does not give the reference to a single text! Therefore, what is the practical benefit of such a list which lacks half of what it is supposed to contain? Finally, although the inscriptions are very few and very short, it is quite an exploit for King's list to succeed to be both inaccurate and incomplete.

On the one hand, her list distorts 2 names, viz

"ʾb(š)" of KiWM 5 (p. 39, 3 times) becomes "ʾbš" (pp. 53A and 54A) and "m(ᶜ)b" = "mᶜb" of KiWM 19 (p. 52) become "mᶜb ?" (p.54B;cf.already above, p.166)

On the other hand, the list excludes not only her reading of KiWM 11 (p. 39), "hbdš," but also 3 elements of KiWM 10 (p. 39), viz]ᶜ, ḫ[and]t.

C - My final assessment of Geraldine King's work is "the ultimate fate it so rightfully deserves, viz that [her work] be withdrawn from any further sale and either be sent to the power hammer or be shredded" (cf. my Miscellanées ..., XVIII [1995], p. 47, on A.J. Drewes - R. Schneider, Recueil des inscriptions de l'Éthiopie des périodes pré-axoumite et axoumite, Académie des Inscriptions et Belles-Lettres, Paris, 1991, 2 vol.). The reader is, of course, aware that "Dr G.M.H. King" was given the secretariat of the Seminar for Arabian Studies (London) (cf. PSAS, 23 [1993], p. 116) from "Ann Dayton" in l.c., 22 (1992), p. 104, a normal promotion after her Ph.D. This promotion, however, added more oil to her weathercock: her name became "Geraldine King" in l.c., 24 (1994), p. iv, and "Dr. Geraldine King" in l.c., 25 (1995), p. ii.

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August 1, 1993 and recently revised.

This note was forgotten during the final setup of the paper and must be added to above, p. 169, immediately below l. 11.

(3) King's doubt about the reading of ltᶜ in Tap 31, comes from her own basic error of giving the value of "l" and "š" to the same sign | , as already stated above, p. 150. Harding correctly reads "ltᶜ" (cf. ADAJ, 17 [1972], pp. 8A and 13B in "Word List," and facsimile in pl. 1; cf. also my Miscellanees..., VIII [1979], p. 5, a publication not listed by King, p. 54). Harding, however, does not hesitate to suggest "perhaps read (y)tᶜ?" (not listed in p. 14B of his "Word List). This suggested correction is wholly imaginative, viz to read | as ʔ . This example shows that for the group mainly represented by Knauf - Winnett - Macdonald - King, what counts is what they have in mind instead of what is on the original which they unceasingly strive to force to their preconceived concoctions.

VI

A 692, YM 362 and F. Bron.

In AION, 41 (1981), p. 164, F. Bron suggested that the funerary slab A 692 published by H. Tschinkowitz-Nagler in Kleine Fragmente (II. Teil), Vienna, 1975, p. 22, "est probablement identique avec la stèle Ṣanʿāʾ 54 published by W. Radt (cf. Katalog der staatlichen Antikensammlung von Ṣanʿāʾ und anderer Antiken im Jemen, Berlin, 1973, p. 13 and pl. 21, upper left) and read by W.W. Müller as 'rʾbt';" this stela is YM 362. The name of A 692 is šʾbt. Bron fails to explain the reason(s) upon which he bases his opinion. All that he had to start with is the photo of YM 362, the short, incomplete description of A 692 by the editor, and his gratuitous assumption that H. Tschinkowitz-Nagler had misread r as š!!! Thus, Bron's opinion can be explained only by blind self-conceit giving the supremacy to his own concepts upon the facts, beside a haughty attitude toward the editor of A 692. Although both antiquities belong to the same type of stela, a simple glance at them or at their photos show them to be different. Here are the necessary details:

	A 692	YM 362
measurements:	29 x 18 cm	22.5 x 12.5 cm
text:	šʾbt	rʾbt
dimensions of the text:	10.6 x 4.7 cm	18.5 x 6 cm.
broken-off corner:	lower right	upper right

VII

C. Robin's misrepresentation of the SA verbal forms.

It is during some lectures devoted to grammatical questions in the frame of "une initiation à l'épigraphie sudarabique" (cf. Raydān, 3 [1980], p. 189) that C. Robin revealed to the world his great discovery, viz "une nouvelle forme de la 3e personne féminine pluriel de l'accompli," (the underlining is mine) viz qtly; cf.

rtdy of CIH 330/3 (cf. below, p. 206);

"h[q]nyy" of CIH 581/3-4 (cf. below, p. 206) and

"hqnyy?" of Ir 34/(2) (cf. below, p. 208).

He also refers to another form already known (p. 190), viz qtln in

Sab Ja 735/9 (cf. below, p. 185);

Qat AM 757/"3 et 7" and AM 208+177 (cf. below, p. 214); and

Ḥaḍr Ja 919/5 (cf. below, p. 224).

Those lectures were published in a paper which, printed in Mas-Gellas, 1983, pp. 173-184, followed by 2 short summaries in Arabic (p. 184) and in English (p. 185), incorporates the above list of entries and claims "to come across new forms that up until now were inadequately analysed" and also "to correct mistakes in the grammars" (p. 185). The reckless temerity of such sweeping statements which truly depicts the man, is backed up by numerous contradictions and errors of all kinds accumulated in those 11 1/2 pages of presentation which the following lines denounce publicly.

A - Sab (cf. Mas-Gellas, l.c., pp. 173-180).

1 - qtlk = "2nd masculine person" (cf. l.c., pp. 173-174).

Robin naturally endorses but only as "probablement" (without any justification, of course!) Jacques Ryckmans' interpretation of rḥmk of Kawkab 1/11 as the verbal form of the 2nd masculine person of the perfect and translated as a present, viz "tu es compatissant" (pp. 173-174), and BeSG (p. 14, 5:2 and [no cross-reference] p. 19, 7:1e) accepts that rendering as certain! what a touching example of loving solidarity!!!

The whole pericope reads: wtrḥm/ʿly/kl/ʿlm/rḥmnn/rḥmk/mrʾ/ʾt.

The 1st obvious objection against Ryckmans' interpretation is that he dissects a short pericope into 3 sections, a fact which he does not even mention and, therefore, he does not even attempt to justify that such a dismantlement is unavoidable. The interpretation of trḥm depends on that of rḥmk. To take advantage of a peculiarity of some modern Yemeni dialects, where qatalka is used for qatalta, is anachronistic unless it is proven - but it is not and cannot be proven - that that unusual form should be applied to this

very inscription. It is the duty of anyone to remain on the solid ground of general Semitics and in particular of SA where the verb sometimes governs a cognate complement (e.g., the well-kown expressions sb³t/sb³, ml³/stml³, etc) and, therefore, to take rḥm as a noun (cf. already G. Ryckmans, in Le Muséon, 60 [1953], pp. 298 and 300, and BiOr, p. 485) and the direct complement of the imperative of the 5th form trḥm (cf. Ar raḥima, 5th form taraḥḥama "to show mercy") instead of the perfect of the verb, as do the 2 aforesaid authors because it is difficult to attribute an activity to rḥm. BeGS (p. 14, end of fn 21) also accepts that trḥm "may also be imperative." How generous! The last 2 words of the text are a hāl expressing both a permanent state and the reason justifying the preceding request. - kl/ᶜlm without article or mimation; cf. Ar ᶜālam. - The periscope may be translated as follows: And shower upon all humans, Raḥmanān, Your mercy, [the] Lord [being] You!

2 - gtl = "masculine dual gtly" (cf. Mas-Gellas, l.c., pp. 173-174). Robin states that gtl stands as the defective writing of dual gtly "dans un texte au moins" (p. 174), viz RoRycBG 9; he also refers to CIH 496 and Ja 2354. Where is the proof? Of course, Robin does not give any because, as it is repeated above (p. ex., pp. 47, 48, etc); "son bon plaisir qu'il ne doit d'ailleurs - telle est sa conviction - ni expliquer ni justifier à personne," or
"Robin ne se doit ni d'expliquer ni de justifier ses caprices à personne,"

a - Gl 1720 (A 607) = HoRycBG 9.

(1) The 2 coauthors republish Gl 1720 in ABADY, I (1982), p. 114, without any photo!!! JaMAR (XIV[1982]) has already denounced vigorously the hypocrisy of Robin's statement, viz "une publication d'inscription sans photographie est inutile" released in his thesis, RoHT, I, p. 90, ll. 11-12; it further shows that this publication is not the 1st where Robin repudiates his own personal oracle; and I have once more denounced (cf. above, pp. 46-48) Robin's loathsome attitude in his Inventaire des inscriptions sudarabiques, Tome I, Paris, 1992. In the present case, however, Robin adds that the squeeze made by "La Mission" "n'est pas mieux lisible" than Glaser's photo of pl. 9B. Does he suggest to the reader that "La Mission" was unable to take a good photo of either the text itself or of its squeeze? Who expects a perfectly clear photo be taken of a damaged or worn-out inscription? Could all photos ever taken possibly be worst than many published in C. Robin's thesis, RoHT, II, e.g., pl. 12B, 18A, 21A, 28, 29,34B, 46B, 50A, 63B and 68C? What excuse does he have for not reproducing his (or someone's else's if he is not capable of drawing some lines) facsimile of the text, as he does with RoRycBG 2 (p. 11) which is another text without photo? Is the photo of the latter not published because it is a mere "graffite rupestre"? If so, the coeditor's attitude

would indeed show no respect for a relic of the past.

(2) The 2 coauthors state that "il y a peu à ajouter au commentaire de Maria Höfner" (p. 114). They do not even mention that Höfner suggests "vielleicht ein Schreibfehler" in rtd because of the singular verb following 2 subjects (cf. l.c., p. 68). There is no engraving error at all. But, the coauthors' main objection is to be found 4 pages before, viz p. 110, fn 13; and p. 114 does NOT refer to that note!? Furthermore, the coauthors' initial remark, "Bḫy: c'est le b (et non le ḫ) qui semble peu sûr" is directly against Höfner's remark that "Die Lesung BḪY is wahrscheinlicher als BHY" (cf. Inschriften ..., p. 68). But the coauthors' uncertainty doubtless is a mere sham because the letter is presented as absolutely certain in both their transliteration and translation. In fact, however, "bḫy" is a reading error instead of /lḫy, the upper diacritic appendix of l is clear on the squeeze. What did confuse Höfner and the 2 coauthors is that lḫy is introduced by a word divider, and all those authors instinctively connected the 2 vertical strokes together into a b. Unfortunately, the upper extremities of both the separator and l are clear of any connecting bar, "N'en déplaise, Son Excellence Robin!"

(3) The singular verbal form rtd is not at all a defective writing, as Robin claims, but a simple case of the verb accorded with the 1st of the 2 coauthors of the text, husband and wife, lḫy/whyrhmw. It is the husband who is responsible for all actions taken by the couple because the authority rests solely on him. Furthermore, Robin erroneously justifies his thesis by nfshmy (l. 2), to which he could have added wldhmy and bythmy (l.4). The dual pronoun is justified because each one of the coauthors has his own nfs, and the children and the house are theirs, not his (= the husband's) only.

(4) RoRycBG 1/3-4 (cf. l.c., p. 108, and photo in pl. 38d; the text is illegible in pl. 38a). Does the pericope hqny/bn/hwbs/²ln/mqldtn indicate bn/hwbs to be a divine name? The 2 coauthors' translation of it as "au 'Fils de Hwbs'" (pp. 109-110) indicates that "Fils" is singular and introduced by the definite article, while the 2nd coauthor speaks of "a 'son'" (cf. RycRSA, p. 172A), thus with the indefinite article proving that there were at least 2 sons and that the so-called dedication was made to one of them. In fact bn could be singular, dual or plural. And contrary to their claim, their translation is not at all proven by their mention of the 2 personal and divine names ²bᶜttr and ²mᶜttr. That the individuals believed that the deity was their father does not and cannot, of course, prove that they actually were the sons of their god. The same belief is also attested in the Ḥaḍr expression mentioned by the 2 coauthors, where the author of the text calls ᶜttr ²bs his father. Furthermore, they could also have referred to, e.g., RÉS 3902, 137/5: wgb/²bs/wddm has ordered his father, Waddum. The 2nd above-listed name, ²mᶜttr, is another name of the solar goddess (cf. JaP, pp. 106 and 112). Therefore, Hawbas being another name of the lunar god ²Ilumquh, bn/hwbs could designate the son(s?) of

that lunar god. The main difficulty unfortunately is that, since that god would be related immediately to the 1st god of the triad, it seems at 1st sight most surprising that he would apparently have no personal name. This difficulty which the 2 coauthors do not even mention, justifies rejecting Robin-Ryckmans' interpretation of bn and looking somewhere else for a solution.

Gl 1720/2-4: -/rtd/ʿttr/whwbs/w(3)ʾlmqh/wbn/hwbs/nfshmy/w(4)nfs/wldhmy/wd/bythmy;

RoRycBG 1/3-4: -/hqny/bn/h(4)wbs/ʾln/mqldtn/-

In these texts, bn is understood as the verb bny has built with the apocope of the final semi-consonant -y; e.g., as in mbn of RÉS 4127/2 = mbny, and bn = bny in CIH 496/1 and2. This suggestion demands the meaning of stela for nfs (e.g., CIH 720/1) which could not be translated as person since nfs (twice) and d are singular introducing the dual -hmy (= husband and wife, lhy and hyrhmw) and the complex bythmw. Furthermore, the asyndetic list of 2 verbs is a well-known feature; cf., e.g., gwm/bny of Sab RÉS 4635/2 = Ja 538 /2, and Qat Ja 343 A/2; cf. déjà plus haut, p. 88.

The 2 pericopes may be translated as follows:

Gl 1720/2-3: - has entrusted [subject = the husband lhy] to the care of ʿAttar and of Hawbas and (3) of ʾIlumquh, and has built for Hawbas the stela of them both [= the husband and his wife] and (4) the stela of the child(ren?) of them both and that [= the stela] of the house of them both.

RoRycBG 1/3-4: - has dedicated [subject= mrfdm] [and] built for Hawbas these cisterns -

(5) The 2 coauthors compare bn/hwbs with the expressions "Les filles de ʾl" and "ʿbdy/-ʾlmqh "les deux serviteurs de ʾlmqh" (pp. 110-111).

(a) Robin repeats his interpretation of the 1st pericope in a text engraved on a small, damaged incense altar from "Sūna L 1" in J.-F. Breton et al., Wādī Ḥaḍramawt. Prospections 1978-1979, Beyrouth, 1982, p.113; the tracing of the letters of the facsimile of p. 112 is particularly inaccurate; the publication of the text is by C. Robin; the color photo of the front is on the front cover; and the date of the leaflet is printed in p. 54. Robin's translation of the short text reads as follows:

"n b hᵐ (nom de femme) a dedié (ceci)
aux filles de ʾil pour son fils."

The text reads as follows:

1	nbḥm/	hqnyt	2
3	bnhtʾl	hbnt	4

1 Nabḥum
2 has dedicated
3 some daughters of ʾIl
4 for her son.

A final note on the preceding Ḥaḍr text. - In Robin's transliteration, add a word divider at the end of l.1; in his translation, correct "nbhᵐ" to nbḥm and "t" of l.2B to ṯ. Of course, Robin does not justify or explain or in any way comment his translation. In both places, however, he introduces the strange novelty of transliterating ʾ as a mere blotch above the line! Cf. above, p. 51. — "The dessin Remy Audouin" of pl. IV is most approximative and unworthy of a professional drawer (cf. also above, p. 180).

(b) The last reference of bnt/ʾl is A.F.L. Beeston in CIASA, I, p. 221: YM 470, which reads as follows on the left, with my translation on the right:

ʾmt/hqnyt/b	1	ʾAmat has dedicated [1] one
nt/ʾl/lbnth	2	daughter of ʾIl FOR her daughter
qhṯm/ʾmt/ʾ	3	Quhṯim, maidservant of ʾA-
hnyn/mqṯ	4	hnayân [AND] [2] this incense burn-
rn/lwfyh	5	er FOR the safety of them
my	6	both.

Cf. already my review of Beeston's publication in JaCIASA, p. 277, and the testimony of the great flatterer, Jacques Ryckmans, copying (what else can he do?) Beeston and, therefore, pointing out that the text "mentionnant les bnt ʾl désigne explicitement l'objet de la dédicace" (cf. JSS, 25 [1980], p. 202)! — Beeston does not point out the palaeographical fact that the 5 m's and the 2 l's of the text are dextrograde in a sinistrograde inscription. — The editor does not make any remark on the feminine name qhṯm, as he should since there is no Ar root such as qhṯ, and the root qṯm is of no help because the qhtl derivation does not exist in ancient Ar. The name is a qtlqtl derivation; cf. qh in JaP, p. 62, fn 13, and ṯm, cf. Ar ṯimm "water, or much water" (cf. LaAEL, p.1877C). — The editor's translation transliterates the personal name ʾhnyn as "ʾḤNY," thus eliminating the final n. Finally, he does not find any etymology suiting the name. It is a ʾqtln form derived from hny; cf. Ar hanâ (i) "to cut, severe (the trunk of a palm-tree)" (cf. LaAEL, p. 819A). - The syntax of the text is obvious; yet, none of the 3 members of the laudemus invicem club, Beeston, Ryckmans and Robin, seems to be able to see what is staring them in the eye, viz the feminine verb hqnyt governs 2 direct complements asyndetically following each other and having each its own particular purpose, viz the parallelism of the 2 sections is perfect:

hqnyt + 2 direct objects { (1) bnt/ʾl ; (2) mqṯrn } purpose: l- { bnth qhṯm ; wfyhmy }

The characteristic of this text must be strongly emphasized, viz the asyndeton was used between 2 verbs (cf. above, p. 180), but also, as here, between 2 direct complements, each of them being followed by its own determination; viz

(1) (ll.1-3): one daughter of ʾIl FOR the authoress' own daughter, and
(2) (ll.4-6): an incense burner FOR the safety of them both, viz both the authoress and her daughter.

(c) One will add to the record of ʿbdy/ʾlmqh the grossly erroneous translation of CIASA, II, p. 211; cf. already my remarks in JaMAR, XVI (1988), p. 13. The pericope hqny/ʿbdy/ʾlmqh is parallel to that of hqny/bnt/ʾl.

(6) Finally, the 2 coeditors refer to "CIAS 39.11/r1 où on a peut-être mention d'un frère de ʾlmqh" (p. 111).

(a) It takes an unusual allucination for sigla of extra queen-size, repulsive length to use the above siglum instead of the very simple YM 441 (cf. CIASA, I, pp.87-89). Unfortunately, however, Robin, having endorsed J. Pirenne's barbarous listing for political reason, has become THE unequalled protagonist of such rebarbative sigla; cf., e.g., " Robin - al-Balaq al-Ǧamūbī," which was called by JaMAR, XIV (1985), p. 269: "the second most obvious nonsensical extravaganza of his publications."

(b) The following notes may be added to my review of A.F.L. Beeston's edition in JaMAR, XIV (1982), pp. 261-262. The present text has as coauthors 2 women whose relationship is not spelled out, but the 2nd woman is not individually referred to in the balance of the inscription. The feminine plural hqnytw with 2 women as coauthors is corroborated by the plural pronon -hmw of lhʿnnhmw (l.3), the only 2 plural forms of the text. The masculine pronoun -hmw may be explained by the fact that the 2 women included in their prayers the other members of their families. In the balance of the text, the pronoun -hw is either masculine with the lunar god as antecedent in dšfttthw (l.7)(the singular verb refers to the 1st authoress only) and in mḫrmhw (ll. 7-8), or feminine in lbhw (l.4), ʾhhw (l.4), lḥmrhmw (l. 5), yhtmrnhw (ll.5-6) and ʾlbshw (l.8) with as antecedent the subject of šftt (l.7), viz the 1st authoress. Beeston considers -hw of lbhw as referring to the lunar god. This understanding is disproved by the fact that it would be quite unusual for a devotee to request a god to be protected against him. One may refer to Ja 722/b-d, wlyḥmrn/ʾlmqh/-(c)--/ʾmhhw/hwn/lhn(d)lbhw and that may vouchsafe ʾIlumquh - - - to His maidservants to soothe for them [= women] His heart (cf. JaSIMB, p. 205A-B). Such a request does not at all presuppose any anger or anything similar in the god; it is a nice way to ask the god to look favorably upon the women. Furthermore, Beeston translates ṯġm as "illwill;" cf. also SDDS, p. 116: "illfavor, disfavour, spite." These 3 words express the result of ṯġm, which is anger. BiDO's meanings of Ar raġama and raġm which she claims to be in "Lane/1114B" are not where she states them to be. The expression bn/rġ(4)m/lbhw/wrġm/ʾhhw may be translated as from the anger of her heart [= her own anger] and her brother's anger.

(2) b - The 2nd text referred to by Robin is CIH 496 = RoRycIA 30 (cf. Raydān, 3

[1980], pp. 162-166).

(1) It is stupid and erroneous (cf.above, p.47) to put between brackets, as if they were restored, all letters already read before. Those remaining on the stone are underlined; this is already quite enough to distinguish them from the others. It would have been more appropriate to pay homage to J. Halévy/Ḥ. Ḥabšūš, whose copies (cf. JA, 19 [1872], pp. 225-226) are remarkably accurate to the extent that they needed only 2 corrections of letters (ll. 3 and 6), the suppression of 1 separator (l. 8), the addition of another (l. 9), and the redistribution of ktlm in 2 lines (ll.10-11). Furthermore, the facsimile of pl. 30 shows such an approximation and a complete distortion of the proportions of the letters that the drawer surely did not even attempt to do a decent job. Moreover, the coeditors' despicable habit of skipping all separator induces the reader into error; e.g., the reader would not expect a separator at the end of both ll.11 (visible on pl. 27B) and l.2 (clearly attested in Hal 630/2).

(2) Robin-Ryckmans' commentary of the text (cf. Raydān, l.c., pp. 164-166) is devoted exclusively to the material reconstruction of the different fragments and does not contain any grammatical or syntaxic remarks. However, their translation makes it clear that, for them, nbṯkrb + nbṯyfʿ are the sons of qwmm + dʾbm (p. 164). This hypothesis making qwmm and dʾbm husband and wife is most unusual and unacceptable. On the contrary, nbṯkrb and nbṯyfʿ must be understood as husband and wife because they open the inscription; cf. also RoRycBG 9 (cf. above, p. 179). Thus, dʾbm is a member of the same family having an unspecified relationship with the couple. Then, bn (l.1) is the defective writing of bny, and bny (l.3) is a plural. This interpretation is suggested by the difference between bn and bny. Were dʾbm the name of the 2nd family beside qwmm, then bn would have exactly the same antecedent as bny, and their different writings would remain unexplained. There are, thus, 3 authors of the text. But, here again, the 1st-mentioned person being the husband is the auhority of the family who takes the responsibility of the engraving (cf. above, p.179); therefore, the singular of ʿbd (l.3), hqny (l.3), and -hw of šymhw (l.5) is perfectly normal. Consequently, Robin's thesis of hqny being a possible defective form of the dual is unsupported by the text. Finally, Robin's statement according to which hqny "peut être analysé comme un singulier" is erroneous because there cannot be any doubt that it is a singular. However, if he brings the text under the caption of "ʿʿl," this means that he is fully convinced that he considers it to be a defective writing of hqnyy, and he is wrong.

c - The 3rd example quoted by Robin is Ja 2354 (cf. JaMAR, II [1971], p. 88), which may be compared with the preceding text.
In this text, a man with his 2 sons, kbr superintended 5 operations, including tqdm/

ʾrdhw the menagement of his helpers, and hṯl he has carried out. The sole personal pronoun of the text, -hw (ll. 4 and 5, twice) refers to the father. Robin, however, takes tqdm as a verbal form. This gratuitous hypothesis, however, breaks up the enumeration of the different jobs of the author as kbr. Finally, Robin also erroneously brings the text under the heading of "fᶜl, 3e pers. masc. duel" since 3 (not 2) persons are listed at the beginning of the inscription, viz hwfᶜm/- -/wbnh/h(2)lᶜw/whṯbm, and all of them are ʾlht (plural) of the ruler.

The present text is also interesting from a social point of vue since it stresses the dependence of a kbr upon a gyl (cf. gyln of l.2).

3 - (qtlw) "masculine dual" (cf. Mas-Gellas, l.c., pp. 174 and 179-180).

Robin's 3rd thesis is "(fᶜlw), 3e pers. masc. duel" in texts with 2 authors and "tous leurs accords au pluriel" (p. 174; the underlining is mine).

a - His example is Ja 641. - This text may be summarized as follows:

- 2 men, father and son (ʾAbkarib), are the authors of the text;
- the plural personal pronoun referring to them, -hmw, is repeated 6 times (ll. 4, 6, 10, 11 and 15); and most spectacularly
- the pericope of ll. 7-9 manifestly proves that Robin eliminates the most important epigraphical fact (cf. also the case of Ja 402, below, the Ḥaḍr section) to save his fantasy.

As it stands and without understanding the plural verbal form as a singular referring to the main subject, it is humanly impossible to comprehend the following pericope, viz

s(8)ftw/ʾlmqh/lwfy/brwHw(9)ʾbkrb

THEY [=father in plural of majesty] have promised to ʾIlumquh for the safety of HIS son ʾAbkarib.

The pericope proves that the plural verb manifests both the plurality of the authors and the unicity of the accountable with whom the -hw concords. Yet, Robin states "(fᶜlw), 3e pers.masc. duel," and his commentary does nothing but repeat what has been known for a long time, viz "ces textes reflètent la tendance, commune à de nombreuses langues, à remplacer le duel par le pluriel; ils n'attestent pas reellement une forme particulière de duel."

How is it possible to more clearly destroy his own thesis in the very same paragraph? How is it possible to contradict oneself so openly? But, Robin does not seem to be aware of it. Furthermore, the parentheses of his "(fᶜlw)" are but a mere gimmick because the same thesis is reaffirmed and defended a few pages later, pp. 183-184.

It may be of some interest to point out that for HöASG (p. 181), a plural verb with 2 subjects or a singular verb with several subjects are considered as exceptions to the

normal rule of accord. While BeDGSA (p. 68, par 58:1) speaks of "frequent laxity in dual accord ... plural verbs and pronouns when strict concord would require a dual," BeSG (p. 17, par 6:2) is plainly content with the mention of "a limitation" of the general rule, viz the case of a verb which "may appear as sg when it has a subject phrase consisting of two or more nouns linked by w, the first of them being a sg." Both statements are inaccurate.

b - Since Robin admits that qtlw is after all a plural form, it is in this place of his paper that he should have added what appears to be an illogical appendix to his Sab section (pp. 179-180) because it deals with the same plural form qtlw: he refers to A.F.L. Beeston's opinion according to which ᶜkrw of CIH 581/9 and sfḥw of Ja 735/8-9 are masculine with feminine subjects. - For the 1st text, cf. JaMAR, XII (1982), pp. 17-28. - The subjects of sfḥw of Ja 735/8-9 are the same as those of sbʾw (ll.7-8), viz kl/śᶜbn/sbʾ/wbnt/mrb the whole tribe of Sabaʾ and the daughters of Mârib. There should obviously not be any problem; cf. below, pp. 185-186.

4 - gtltw = "feminine dual" (cf. Mas-Gellas, l.c., p. 174).

Robin's "fᶜltw," dual of the 3rd feminine person (p. 173), is illustrated by him (p. 174) in Ja 686 and YM 441, and BeSG reproduces the same information (p. 14, par 5:4)

a - Ja 686. - Here again, everything is quite normal, viz 2 women introduce their social condition with dty (feminine dual), and they are the subjects of the plural feminine verb ḥqn[y]tw; the feminine pronon -hw (ll. 4-5) refers to the 2nd woman because of ll. 5-6; and the masculine plural pronoun -hmw (ll. 6 and 7) refers to the 2 women and the child mentioned in l. 5. The feminine plural form ḥqn[y]tw is listed as such in JaSIMB, p. 434A and partly restored in Ja 722/3-4. All is normal.

b - For YM 441, cf. above, p. 182. It is incomprehensible that Robin would maintain his theory of the dual in spite of his statement reproduced above, pp.184-185.

5 - gtln = "feminine plural." The next item is Robin's paper on "fᶜln" (pp. 175-176).

a - The 1st text considered by Robin is Ja 735/8-9 which reads as follows:

/wsf(9)hw/rqthmw/wtᶜrbn/lmrʾhmw/ʾlmqh/wʾntn/ᶜṯwfhn, and the edition's translation is: "and they (9) displayed their sorcery and they have given pledges of submission to their lord ʾIlumquh and [also] loving wives" (cf. JaSIMB, p. 212A and B, respectively).

The correct understanding of this pericope demands the mention of ll.7-8, viz the phrase immediately preceding the above-listed quotation:

wsbʾ(8)w/kl/śᶜbn/sbʾ/wbnt/mrb/bᶜbr/ʾlmqh/ᶜdy/mḥrmn/dʾwm

and have contended (8) the whole tribe Sabaʾ and the daughters of Mârib against ʾIlumquh in the temple, the one of ʾAwwâm (cf. l.c.).

Thus, both verbs sb²w (ll.7-8) and sfḫw (ll.8-9) have the same subjects, viz kl/s̃ᶜbn/sb²/wbnt/mrb (cf. already above, p. 185); THIS REMARK IS OF THE UTMOST IMPORTANCE.

(1) I wish to take advantage of the present study to at long last denounce the overall handling of JaSIMB's manuscript: it was thoroughly revised and altered without my knowledge and/or previous consent. Even the publication of the inscriptions was mishandled; I was presented with a final text which I (at least) could not change. A manifest case of mishandling of manuscript is the location of Ja 851, where the self-proclaimed αὐτόκρατωρ editor was finally forced to print

"Addendum [Due to editorial misunderstanding, Ja 581 was placed here, following Ja 850, rather than after Ja 761 (p. 228) as the author intended],"

a simple phone call would have prevented his crazy initiative; he was also forced to add note "851 - See addendum, pp. 251-252" (cf. JaSIMB, pp. 251B and 228B, respectively). The text of my translation of Ja 735 also was altered; alas!, it is not possible for me to restore the original text since I do not even have a carbon copy of the original. But the carbon copy of the original glossary is still at my disposal. All "(A)", "(B)" and "(C)" are unwarranted additions to the manuscript; the order of the original lines was disturbed; and the "Explanatory Notes" at the beginning of the glossary (cf. JaSIMB, p. 426) are from that editor who substituted his own text to mine with, of course, the necessary details explaining his changes as if they were in the manuscript; e.g., his (5).- As far as I can remember, I have always considered the verb sfḫw as governing 3 direct complements, viz rqthmw, tᶜrbn/lmr²hmw and ²ntn/ᶜṯwfhn. My original translation must have been something like this:

and they [= the whole tribe Saba² and the daughters of Mârib] have displayed their sorcery and the giving of pledges of submission to their master ²Ilumquh and [also] the loving wives in the temple, that of ²Awwâm.

(2) My original understanding of the text was as follows. The whole pericope sb²w/kl/s̃ᶜbn/sb²/wbnt/mrb/bᶜbr/²lmqh implies that, faced with a severe drought, the whole population of Mârib finally decided to embark on all-out effort to somehow force the attention of their god to answer their prayers for rain. The very mention of bnt/mrb may be explained by the fact that the expression kl/s̃ᶜbn/sb² does not specifically include the womanfolk because it often actually excludes them especially in the narrative of military expeditions. The most the whole population of Mârib, both men and women alike, could do to that effect was to muster an exceptional display (sfḫw) inside the great temple itself (ᶜdy/mḫrmn) of ²Awwâm, of all means at their disposal, viz their sorcery (rqthmw), the giving of pledges of submission to ²Ilumquh (tᶜrbn/lmr²hmw/²lmqh) and, finally, the hierodules (²ntn/ᶜṯwfhn) in full force, not only a few of them only. Of course, we do not know either what the arsenal of that sorcery actually contained or what

those pledges of submission actually were. At any rate, the whole population of Mârib, men and women alike, met with full success.

(3) A few years after the publication of the text, M. Höfner released her translation of the pericope after noting that a severe drought had prompted the tribe of Saba² and the daughters of Mârib to go in "Prozession" to the temple of the lunar god, "dehnten sie lang aus ihre Beschwörungen und die Darbringen (von Opfern) und die Frauen ihre Umhange (ṯawaf)" (cf. Religionen Altsyriens, Altarabiens und der Mandäer [Religionen der Menschheit, 10, 2], Stuttgart, 1970, p. 338).

Höfner overlooked the ᶜ of ᶜṯwf in her translation which refers to "ṯawaf" (from ṯwf), but she correctly understood qrthmw, tᶜrbn and ²ntn as direct complements of sfḫw, but since the subjects of sfḫw are the whole tribe of Saba² and the daughters of Mârib, the article -hn of ᶜṯwfhn cannot possibly be a feminine pronoun. Finally, the idea of a procession is disproved by the fact that the above pericope describes the activities inside the temple as ll. 7-8 ,as it places all devotees inside; cf. ᶜdy/mḫrmn of 1.8 (cf. above, pp. 185-186), and below, p.215, the note on bmḫrms of AM 757/4-5.

In 1966, Jacques Ryckmans had nothing to say (is it possible?) but that ²ntn/ᶜṯwfhn were "formes intéressantes dont l'analyse reste douteuse" (cf. Le Muséon, 79 [1966], p. 499)! Of course, Ryckmans was careful enough not to justify his doubt!

(4) Ten years later, in 1976, MüNAI (p.62) published this laconic statement: "tᶜrbn (sofern nich Infinitiv der V. Form) 'sie (pl. fem.) gaben ein Pfand'." But, as was fully expected, no explanation or justification is volunteered; e.g. who are those feminine subjects?; and no translation of the whole phrase is given. Müller gives an outstanding example of what is most common among the laudemus invicem club members, and very specially Beeston, viz the analysis of one word without paying any attention to the context. Indeed, tᶜrbn cannot be separated from its complement, lmr²hmw/²lmqh. It is, then, most obvious that, if tᶜrbn is a feminine verbal form, viz having women as subjects, the masculine pronoun -hmw of mr²hmw is grossly erroneous for -hn. But, Müller apparently does not care about epigraphical evidence, provided his pruritus of sensationalism and showing off is quelled as it has always been the leitmotiv of his life.

(5) In BSOAS, 35 (1972), p. 352B, published in 1976 since that page refers to Jacques Ryckmans' abovesaid statement, A.F.L. Beeston actually is the 2nd to deal with that pericope.

(a) Beeston never informs his reader that my interpretation of -hn of ᶜṯwfhn is that of being the article.

(b) Beeston labors the phraseaology of JaSIMB's translation. His critic does not concern me.

(c) Then, Beeston refers to Jacques Ryckmans' above-quoted statement which, according

to him, "mentioned the difficulty of analyzing the morphology of ʿṯwfhn." Here again, Beeston misunderstands what he reads: Jacques Ryckmans' text does not say that the analysis of ʾntn/ʿṯwfhn was "difficult." It is so obvious that it would be ridiculous to labor the point.

(d) Beeston refers to W.W. Müller (cf. AfO, 21 [1966], pp. 104A-109A), who, according to him, "has observed that here (as also in rḫlhn of Ja 649/40 and 665/39) we have the feminine plural pronoun affix." Here again, blinded by his own theory, Beeston falsifies Müller's testimony because Müller does NOT AT ALL mention Ja 735. This text is on pp. 211B-213B of JaSIMB, and Müller (p. 108B) jumps from p. 208 to p. 214. Thus, Beeston voluntarily invents and ascribes to Müller a statement which the latter never wrote. It is on p. 107B that Müller publishes his translation of rkbm/brḫlhn of both Ja 649/40 and 665/39 (erroneously transcribed as "brhlhn" by him) "Reitkamelinnen mit ihren Sätteln;" thus, giving -hn the value of the 3rd personal feminine pronoun.

Müller's interpretation which is endorsed by Beeston (there is no -hn entry in SDDS, p. 56) is erroneous because rkbm/brḫlhn is introduced by the number "316" and "1200," respectively, in Ja 649/39 and 665/39, and the context makes it certain that all those animals were a part of a booty. It is impossible for either Beeston or Müller or anyone else to prove that among those 2 groups of camels there would not even be one single male, viz all those 1516 camels would a l l be females. To prove his translation Müller invokes "mehri rikīb, pl. rikōb 'Kamelin'." This feminine exclusivity is contradicted by Mehri "rīkēb/rīkōb 'riding camels," and "rīkēb (rkib) camel" (cf. J. M. Johnstone, Mehri Lexicon and English-Mehri Word-List, London, 1987, respectively, pp. 323 and 499); Jibbālī "ʾerkīb riding-camel" (cf. J.M. Johnstone, Jibbāli Lexicon, Oxford, 1981, p. 211), "rikab mount, camel" (cf. J.M. Johnstone, Eastern Arabian Dialect Studies, London, 1967, p. 140), "rekīb riding camels" (cf. J.M. Johnstone, Ḥarsūsi Lexicon and English-Ḥarsūsi Word-List, Oxford, 1977, p. 104, and also p. 156B), Daṯīnah "rabûb monture ... cheval ou chameau" (cf. LaGD, p. 1385), and Ar rikâb "[travelling-camels, used for riding; i.e.] camels upon which people journey" (cf. LaAEL, p. 1143B) and "noun, Camels" (cf. J. Penrice, A Dictionary and Glossary of the Ḳor-ân, New York, 1969, p. 59B).

A final note on rḫl as attested in the pericope kbr(27)rḫlm in Ja 651/26-27, "Leader of Raḥlum" (cf. JaSIMB, pp. 155B and 156A). Of course, SDDS had to find something else, and this reference is found under the noun "rḫl equipment (e.g. saddle etc)" (p. 116). This interpretation is erroneous on 3 accounts.

(a) kbr is never used in relation with inanimate things.

(b) kbr is normally connected with a clan (e.g., kbr/kmdn of Ja 552/1), a tribe (e.g., ʾkbr/fyšn of RÉS 3913/1; ʾkbrt/kdt of Ja 576/2), a country (e.g., ʾkbrt/ḫdrmwt of Ja

665/33) or a city (e.g., kbr/ṣrwḥ of RÉS 3951/1, and kbr/hgrm of CIH 603/13); it is also followed by a noun designating humans either determinated by the article (e.g., kbr/ʾmhrn of Ja 954/2-3) or in the construct state (e.g., kbr/ʾhl/ʾqrʾn of Ja 422/2, and kbr/ʾʿrb/mlkn of Ja 665/1-2). Therefore, the simple fact that -m of rḫlm is the mimation already proves that the word could not designate human beings.

(c) The intendant of the section "equipment" would not be called kbr, as "the equerry of the horses of the king" was called tly/ʾfrs/mlkn (Ja 584/1; cf. the plural forms tlwt, ʾtly and ʾtlwt). The tly of the royal horses must also have had in his attributions the care of everything needed by those horses, as the "equipment" has no value in itself unless connected with its users.

(e) After endorsing (how is it possible?) my reference of sfḳ to modern Ḥaḍr sfḳ "jeter, verser, répandre" (cf. LaḤ, p. 608), Beeston adds that "I cannot follow him in believing that it validates a 'derived' sense of 'display'; such a semantic derivation seems very improbable."

The 1st meaning of "display" is "to spread or stretch out or wide," and how is it possible to "verser, répandre" anything without spreading it? Really, Beeston shows himself rather dull. Besides, sfḳ was similarly rendered by Höfner, viz "ausdehnen" (cf. above, p. 187). Of course, Beeston does not refer to Höfner's translation! Furthermore, Beeston should be the last to speak of "derivation improbable." One of the best examples of his extravagant etymological research, which was denounced in JaMAR, XII [1982], as "pure imaginative juggling," concerns the verb ʿkr of CIH 581/9 (cf. below, p. 207), viz

> "I cannot help feeling that the most probable way of dealing with ʿkrw is to render it as 'be pregnant'; cp Arabic ʿakira 'be turbid, feculent (of water)', and the semantic link between Latin faeces 'dregs' and fecundus 'pregnant'" (cf. Arabian Studies, 4 [1978], p. 23).

What kind of scientific method may have a person who is unable to master his own feelings? How irresponsible of that person to publicly acknowledge it! And, as soon as his "feelings" sense the wind of total freedom blowing their way without any restraint whatsoever, they launch out into the wild frenzy of their life. Finally, as so often is the case he uses an ambiguous expression, viz "the semantic link." Latin faex is of unknown origin (cf. Oxford Latin Dictionary, Oxford, 1968, p. 672A, and C.T. Lewis and C. Short, A Latin Dictionary, Oxford, 1980, p. 720B) while fēcundus is understood as composed of "ṯfeo" and "cundus" (cf. l.c., respectively, pp. 683B and 732A). But, of course, Beeston knows better than the specialists who wrote those 2 dictionaries.

(f) Beeston considers the last 2 words of the pericope "as a ḫāl clause" and translates them as "while the [other] womenfolk [scilicet the 'daughters of Marib' mentioned in l.8] were their [scilicet 'the sorcerers'] supporters."

Such an analysis can easily be described as plain crazy and idiotic. Those bnt/mrb

are the 2nd suject of the 2 main verbs sb³w (ll. 7-8) and sfḫw (ll. 8-9). How thus can -hn of ʿṯwfhn be referred exclusively to the 2nd subject of the 2 plural verbal forms? It is also erroneous for Beeston to identify those bnt/mrb with the vague ³ntn because the 1st are the true feminine members of the tribe Saba³ while ³ntn is the womanfolk living with the tribe without being members of the tribe by birth. Moreover, how can he refer his gratuitous feminine pronoun -hn to rqt which he also gratuitously interprets as designating women while the would-be antecedent is separated from its personal pronoun by another direct complement determined by lmr³hmw/³lmqh?

(g) The best etymology Beeston could find is "Ar ʿaṭafa 'double over, fold something,' and ʿaṭf 'co-ordination'." The 1st 2 meanings apply to material things, such as, a piece of garment or a stick, while the moral derivation implies "an inclination toward" or "to be favorably inclined toward;" and ʿaṭf "co-ordination" is an Ar grammatical term which is "un cheveu dans la soupe;" but ʿaṭf also means "bend(ing), inclination, sympathy." It takes much more than "sympathy" (which Beeston does not use)for someone to become a "supporter" of that he is inclined to? But, to derive the meaning of "supporter" from the idea of "double over, fold something" is truly a "Beestonian" unequallable, aerial, high-trapezium acrobatic feat.

(6) In 1973, Jacques Ryckmans practically devotes a whole paper to the edification and enlightenment of Annuaire de l'Institut de Philologie et d'Histoire Orientales et Slaves, 20 (1968-1972] 1973, pp. 379-388) to present his interpretation of the pericope. The only worth of this paper is the author's translation of the whole pericope, viz

"et ils (c.-à-d. les responsables de la tribu de Saba³) ordonnèrent à leurs sorcières { et elles donnèrent / de donner } des gages (de soumission) à leur seigneur Almaqah, tandis que les (autres) femmes étaient à côté d'elles" (p. 385).

Beside the author's normal and ordinary verbiage and the statements parallel with,or close,to Beeston's opinions (e.g., p. 381), there are a few statements in need of corrections.

(a) It is immediately obvious that sfḫ does not mean "ordonner" and that his so-called "responsables" actually are "the whole tribe of Saba³ and the daughters of Mârib." Is it possible to be so dishonest? More about those 2 questions below.

(b) It is indeed symptomatic that, although referring explicitely thrice (pp. 379, 380 and 386, fn, 3 and 1, respectively) to his 1966 prose (cf. above, p. 187), Jacques Ryckmans never alludes to his empty verbiage published there.

(c) Jacques Ryckmans begins with a categorical affirmation according to which -hn of ʿṯwfhn "est certainement celle du pronon suffixe nominal de la 3e personne du féminin pluriel" (p. 181).

Of course, no proof is given; neither does he inform his reader that -hn was understood as the article (cf. JaSIMB, p. 433B), an information which Jacques Ryckmans cautiously hides from his reader. Yet, that information is the answer to his problem, viz "on ne ne voit pas comment -hn pourrait être la desinence d'un adjectif féminin pluriel, ou d'un appose à ³ntn" (p. 381).

Furthermore, he claims that "A. Jamme ne donne d'ailleurs aucune analyse grammaticale de ³ntn ʿṯwfhn" (p. 381). J. Ryckmans proves to be a brazen liar;indeed,he flatly denies what is clear in both the translation, viz ʿṯwf is an adjective appended to ³ntn, and also the information "ʿṯwf loving; "hn, 735/9" (cf. JaSIMB, p. 444B) and "hn (A) [article], cf. ʿṯwf, rḥl" (cf. l.c., p. 433B)!Cf. also below,p.223, -hn in Ḥaḍr CaTh 4/6.

(d) Jacques Ryckmans interpret rqt as the plural of the feminine Ar rāqiyat. But he does not even attempt to prove - because he cannot - that my etymology from Ar ruqyat "sorcery, witchcraft, magic" (cf. JaSIMB, p. 213B) is wrong. Yet, he has nothing at all to justify his own interpretation of tʿrbn except his own etymology of rqt. Thus, his whole theory is exclusively based from the start on the rejection of an evident etymology, and the balance of his argumentation cannot make it either secure or safe unless by using a trick familiar to Jacques Ryckmans, viz the latius hos.

(e) Jacques Ryckmans states that "en sabéen nous pouvons citer, pour l'emploi de -hn comme suffixe nominal" (p. 381, fn 2),

9 pericopes belonging to 7 texts.

[1] The French expression "suffixe nominal" is wrong; he should have used the expression "pronom suffixe." One example of "suffixe nominal" is "-able" as, e.g., in "blâmable" from "blâmer;" cf. M. Grévisse, Le bon usage, Gembloux, 1980,p. 99, no.170.

[2] Two of his references, Ja 649/40 and 665/39, are erroneous. Jacques Ryckmans copied his interpretation from MÜNAI (cf. above, p. 187).

[3] His 7 remaining references are these.

[a] - CIH 21/3. - CIH (I, p. 34B) had already noted "mqbrhn = مقبرهن." Thus, here again, Jacques Ryckmans copied his opinion somewhere else.

[b, c] - Fa 3/5-6 and 6. - Jacques Ryckmans fails, of course, to point out that Gonzague Ryckmans took -hn (twice) as a demonstrative adjective, "ces" (cf. RyET, p. 4) and was followed by HÖISA (II, p. 30).

[d, e] - Ja 722 a and c.- The missing section which separates ll. 1-6 from a-e cannot support Jacques Ryckmans' interpretation of -hn of ʿmhn (l.a) as a feminine personal pronoun because ll. 4-5 mention the offering of a male statue, slm; thus, a man (probably a child) was involved and his mention in the missing section would make Jacques Ryckmans' interpretation erroneous. It is for that reason that JaSIMB (pp. 205A and 445A) interprets ʿmhn as an infinitive. In c, the edition already points out: "them [women]." Furthermore, Jacques Ryckmans' phrasaeology may give the reader the impression

that his list is the consequence of his own work and is complete. The preceding remarks show that the 1st impression is not correct at all. The 2nd impression is also wrong: the pronoun was already found in Fa 76/4 (4 times) and 5 (twice).

(f) Jacques Ryckmans' interpretation of bnt/mrb (pp. 381-382) also shows his mind wandering afar with no contact with, but at the expense of, the text itself. This trick allows him to subreptitiously introduce and implant in the reader's mind the essential element of his thesis. Indeed, he claims that "à notre avis, il s'agit d'une classe sans attache tribale spéciale (parce qu'elle était d'origine servile ou étrangère), qui constitutait le monde des sorcières, magiciennes, entremetteuses et prostituées" (p. 382).

One may well wonder why Jacques Ryckmans limits his list to those 4 categories and excludes "courtisanes, devineresses, diseuses de bonne aventure, nécromanciennes, etc." Furthermore, Jacques Ryckmans' characterisation of bnt/mrb is insulting, and is flatly contradicted by the word bnt itself which, as well as bn, denotes always a blood relation with a family or a clan. The bnt/mrb were born at Mârib in Sab families, and could not possibly be foreign to the native Mâribites. Had they been foreigners to the tribe, they would have n e v e r participated in the authority they apparently shared since they are the subjects of the 2 verbs sb²w and sfhw along with the administration of the tribe Saba². This point is of capital importance in the translation of 11.8-9 (cf. above, pp. 185-186).

(g) Jacques Ryckmans reproduces (p. 385) Höfner's translation of the pericope which is repeated above (p. 187), but not without tampering with it by inventing "[...]," immediately after "Opfern)." This distortion of the written word, of which Jacques Ryckmans does not inform his reader, is not accidental because of his unjustified assumption according to which Höfner's translation "suppose sous-entendue une forme verbale féminine de sfḥ devant ²ntn."

Quite unexpectedly, however, Jacques Ryckmans seems to have felt some kind of sorrow or remorse because he immediately adds that "une telle construction nous paraît forcée (on s'attendrait à ce que ce verbe au féminin soit exprimé) - ou encore que notre impression soit subjective et impossible à vérifier."

But, here again, what is Jacques Ryckmans sorry about? As it is well-known, ambiguous statements being a characteristic of his style, as already stated by E. Ullendorff in BiOr, 12 (1955), p. 217, one may ask here: is he sorry about his understanding of Höfner's translation or about his suggestion of a feminine verbal form of sfḥ before ²ntn? But, the 1st is a manifest error, and the 2nd a wasteful hypothesis which is not heard of again in the balance of his paper. Thus, what might be the purpose of such statements if not to pass on to the reader the confusion of his own mind? And ... Jacques Ryckmans keeps on doing the same year after year.

(h) Jacques Ryckmans' next paragraph is to review Beeston's opinion. But what he does is incredible from any normal human being: he quotes as if from Beeston himself a translation which distorts Beeston's words. Beeston's translation is repeated here in the 1st line, and in the 2nd the text that Jacques Ryckmans attributes to Beeston (cf. "la traduction qu'il propose pour ces mots"):

Beeston: "while the [other] womenfolk [] were their [] supporters;"

Ryckmans: "the (other) women being their escorts" (p. 383).

He does not even see the difference between "supporters" and "escorts"; cf. already my Miscellanées ..., XIV (1985), pp. 200-202 for Jacques Ryckmans's amazing "French translations" of SDDS's English terms. Jacques Ryckmans's ego obviously is so great that he considers himself above all rule of honesty and respect of the written word.

(i) Jacques Ryckmans's next topic is the meaning of the verb sfḥ.

[a] He states that "la racine éthiopienne [of sfḥ] a aussi le sens de 'publier'" (p. 384). A. Dillmann's Lexicon linguae aethiopicae (New York, 1955 , col. 402) mentions a single reference as the 2nd application of the transitive meaning of the verb, which is supported by only one source, the book of Esther, viz "expandere vel patefacere epistolam, ut ab omnibus legi possit." Note that S. Grébaut's Supplément au Lexicon linguae aethiopicae de August Dillmann (1865) et édition du lexique de Juste d'Urbin (1850-1855), Paris, 1952, has nothing to add to col. 402 of Dillmann either on p. 226 or p. 454 in the "Addenda." Dillmann's above-listed meaning is reflected by "lay open," the 6th meaning given by E. Leslau, Comparative Dictionary of Ge'ez (Classical Ethiopic), Wiesbaden, 1987, p. 487B , which must be understood as synonymous of the 5 preceding words, the 1st of which is "stretch forth;" and Leslau confirms the meaning of Ge'ez by mentioning of "be wide, be extended." Summarily, Jacques Ryckmans chooses an Ethiopic hapax legomenon for its use in SA in the same manner that Beeston would select an hapax legomenon of the Ar poetry!

[b] Jacques Ryckmans claims (p. 384) that sfḥ would mean "ordonner, décréter" when used "en compagnie de śry" with reference in fn 2 to RÉS "3689, 1; 3691, 1; 3691, 1" and to a 1919 publication of N. Rhodokanakis, "pp. 105-106." Obviously enough, the 2nd mention of "3691, 1" is a dittography. The reference to Rhodokanakis is a cheap way of impressing the reader because he simply copied it from RÉS, and his mention of "pp. 105-106" is erroneous because Rhodokanakis speaks of sfḥ on p. 105 only where the editor's only comment is the mention of that derived Ethiopic meaning, which is duly reproduced in RÉS, VII, p. 270. It is from RÉS that Jacques Ryckmans copied his reference to that Ethiopic hapax legomenon ... and he went to check in Rhodokanakis's publication.

Then, it was of course sound flattering politics for Jacques Ryckmans to go along with the leader of the laudemus invicem gang, Beeston who, in his pamphlet BeLT does not at all hesitate to translate śry/wsfḥ of RÉS 3689/1 as "has promulgated and recorded" (p.10). But this translation of sfḥ, which Beeston erroneously copies "śfḥ" (p. 10) confuses his "śfḥ" with śhf "the registration" (pp. 12-13) of RÉS 3688/1. The Qat verb sfḥ, 1st form, means "to declare binding/legal."

Note on S.D. Ricks' dictionaries and the verb sfḥ. - Ricks finds nothing against Jacques Ryckmans' fallacious study of the verb sfḥ Indeed, RiLEQ (p. 255) has no entry for the 1st form sfḥ, but he refers to Jacques Ryckmans's paper of "1972, 385" in his entry of the 8th form stfḥ. In his bibliography, however, there is no 1972 publication listed under Jacques Ryckmans' name. The year "1972" belongs to the date of the volume number, viz "1968-1972;" but the publication date of the book is 1973. Furthermore, "385" must be corrected to "383-385" (cf. below). RiLIQ (p. 161) repeats RiLEQ, including the doubly erroneous mention of "1972, 385" (cf. above). He also introduces the 1st verbal form sfḥ with reference to the same Jacques Ryckmans' paper, but the reference becomes "1973, 83-85." If "1973" is the correct publication year, "83-85" must be corrected to "383-385" (cf. above).

(j) Let us see, in the section of Jacques Ryckmans' paper , the 34 lines of pp. 384-385 where he deals with Ja 578/21-22 and especially ll. 28-29. Hypnothized by his erroneous understanding of sfḥ, he is happy to unleash his pan-Ar whim by applying to SA a typical Ar expression which he chose from Ar ʾamara on the sole basis of his basic error of sfḥ "'décréter, ordonner'" and for the sole purpose of salvaging his preceding understanding. This is a dirt-cheap latius hos. His conclusion on "le sens prégnant de la préposition ṯbt" (p. 385) is also revolting because such a reality does not exist in the Ar expression he quotes, where a verb is understood. Such an expression is foreign to SA.

(k) After bellowing his selfish victory outcry of "ayant ainsi dégagé la signification de sfḥ" (p. 385), Jacques Ryckmans publishes his translation of the text in 2 stages which are represented in my transcription (cf. above, p. 190) by the 2 braces in the 2 expressions, "et elles donnèrent" and "de donner."

[1] Jacques Ryckmans' translation is contradicted by his understanding of bnt/mrb (cf. above, pp. 190-191), but he does not want to see it : he is blinded by his thesis. His translation describes the subjects of sfḥw as follows: "ils (c.-à-d. les responsables de la tribu de Saba)" (p. 385).

But, as already pointed out above, the sujects of sfḥw are kl/śʿbn/sbʾ/wbnt/mrb (cf. above, pp. 185-186). Thus, Jacques Ryckmans eliminates the 2nd group of the subjects of

sfḥw and, more importantly, he is forced to do it because of the 2nd group which he partly identified with rqthmw (cf. above, p. 192), an identifications made certain by his excursus (p. 382) on bnt/mrb. Confusion and contradictions, which have always been the trademark of Jacques Ryckmans' papers, are illustrated here once more.

[2] The adjectif ʿṯwf is explained in JaSIMB (p. 213B) by referring to "Arabic ʿaṯūf '(A woman) loving to her husband, affectionate to her child(ren)'." The parallelism could not be more perfect. Of course, in order to make very sure that his reader would not be impressed by that parallelism, Jacques Ryckmans does not breathe a word of it. His interpretation of the word calls for "le sens fondamental de l'arabe ʿiṯf 'côté'" (p. 383). But the basic meaning of the root ʿaṭafa is "to be inclined, bend" (cf. LaAEL, p. 2079C). Furthermore, Ar ʿiṯf is a noun, not an adverbial noun. Thus, his translation as "à côté de" is unjustified (Jacques Ryckmans does not even attempt to justify it) because "à côté de" should be bʿṯf, which does not exist. The parallelism wsṭ/bwsṭ is invalid because Ar knows both the adverbial noun wasṭa "in the middle of" and wasṭ "center, middle." In spite of Ar wasṭa, SA normally uses the expression bwsṭ (or bysṭ) while simple wsṭ is very rarely attested (e.g., RÉS 3958/4-5). The case of ʿṯf is different inasmuch as Ar "ʿiṯfa" does not exist.

Finally, Jacques Ryckmans gives the reader a revolting example of his reasoning method when rejecting my translation of ʾntn/ʿṯwfhn as "loving wives;" he writes this:

> "Une telle offrande d'épouses, mentionnée presque incidemment, et qui n'est reprise ni dans la formule dédicatoire initiale, ni dans la conclusion du texte, paraît fort étrange, d'autant plus qu'elle implique l'existence, au temple de Maʾrib, de pratiques d'hiérogamie dont les textes sud-arabes ne livrent aucun indice à l'époque relativement recente et bien documentee de notre inscription" (p. 380).

The dishonesty of such an argumentation is only equalled by its stupidity. His expression "mentionnée presque ... dédicatoire initiale" applies also to his "sorcières" and to his "(autres) femmes." His remark is nothing but idiotic because ll. 7-9 are an incidental phrase in the whole text and, as such, cannot be expected to be found in either the introductory or closing pericopes of the text. Furthermore, his "sorcières" and his "(autres) femmes" designating prostitutes and the like are hapax legomenon in the whole SA material. Finally, the hierodules institution has been well-known in SA for a long time. The preceding remarks show that, when it suits him, Jacques Ryckmans ignores well-known textual facts as well as the normal setup of the inscriptions. He does the same on the question of the double subjects of the verbs sbʾw and sfḥw. He does it again in fn 3 of his p. 380 on Ja 669/14-15 (cf. below, pp. 199-200).

[3] Jacques Ryckmans invents "(autres)" in his expression "les (autres) femmes." No word is given to justify the unwarranted addition which, however, is necessary to his translation. Who are those "(autres)" women? According to his p. 382, those "(other)"

women are, as rqt his "sorcières," "des magiciennes, entremetteuses, et prostituées," all of them bnt/mrb (cf. already above, p. 192). Therefore, all those persons, viz his "sorcières," as well as the others listed above, participated in the authority of "the tribe Saba'," since, it must be repeated here again, ALL OF THEM are those who, with the whole tribe Saba', are the subjects of the 2 verbs sb'w and sfḫw. What an utterly revolting opinion? In addition, what might have been the purpose of those so-called "(autres) femmes"? Were they "by the side" of the rqt to hold their umbrellas or to give them some refreshments once in awhile or to stand in readiness to take over in case the rqt, or some of them, would faint because of the great heat, or to help them as assistants, or to do something else? Sorcerers or sorceresses normally like to be left alone to do their job freely and do not want anybody around except an eventual assistant who always remains in the background.

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Summarily, Jacques Ryckmans suggests to all reader of the Annuaire de l'Institut de Philologie et d'Histoire Orientales et Slaves (cf. above, p. 190) that the nec plus ultra production of his intellect - (Who knows how many hours and weeks Jacques Ryckmans spent to bring to completion even the smallest detail of his thesis because of the special purpose of his paper?) - is the presentation of grammatical and social nonsenses which may be summarized as follows:

(a) the bnt/mrb the daughters (by blood) of Mârib, whom Jacques Ryckmans, scandalously describes as a mere bunch of sorceresses, procuresses, prostitutes, and the like, would have been chosen by the 1st authors of the inscription, viz kl/s̃ʿbn/sb' the whole tribe Saba', as the cosponsors of the public ceremony held in the great temple of 'Almaqâh! How is it possible?

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(a) It is very regrettable and quite astonishing that Beeston does not express his doubts as he normally relishes to do. He could easily have added an extra fn after completing his manuscript, as he did in his fn 94a (p. 51), because, when he wants, he can write very lenghty footnotes, such as 101 (pp. 54-55: 19 lines), 80 (pp. 45-46: 21 lines), 15 (pp. 9-10: 22 lines), and also 74 (p. 41: 14 lines), which he, himself, calls "rather trite remarks"!!! what a degrading avowal!

(b) stnḫs of RÉS 4176/6 is erroneously translated "to be pregnant" (cf. also SDDS, p. 95, but between parentheses) because this verbal expression

translates the whole locution stnḥṣn/bnslm, which means to be heavy with young. SDDS's 2nd interpretation (also between parentheses), viz "be suckling, nurturing offspring," is also wrong because of the preposition b introducing nsl. Note again Jacques Ryckmans' atrocious rendering of the above English expression as "n'être pas encore sevrée (progéniture d'animal)." He apparently does not know the difference between "to suckle" and "to suck;" he might have found out had he attempted to translate "nurturing" instead of eliminating it.

Beeston's hypothesis that the form stnḥṣn "could be regarded as ... imperfect" is contradicted by his par 5:5 (p. 15) where the plural feminine is shematized as "(y-...-n?)." It is indeed very easy to posit that the y- preformant was vocalized; it should be proven instead of being simply stated; and it cannot be simply stated because of a thesis (cf. also immediately below, after the discussion on stnḥṣ). That Beeston considers stnḥṣ as a perfect is but the consequence of his position according to which "Sab k- functions

> prepositionally only in the sense 'like' and not 'for', so that the infinitive interpretation is here excluded" (p. 15, par 5:9),

viz in RoPH 105/5: wkgybhmw in RoPH, pp. 321-322: "et qu'il les a préservés de [...," where "et que" stands for "et [parce] que." Beeston states on that expression that "the only possible rendering here is 'and that He should protect them'."

Beeston's summary on k- does not exactly coincide with SDDS's interpretations which include both past and future, viz "that,

> when, because;
> as;
> so that, in order that" (p. 75).

Robin's translation is justified by the change of conjunctions: ʾIlumquh had ordered His devotees to dedicate to Him the monument (gyfn; l.2) located at a certain place (the text is fragmentary) for 2 purposes, viz. lsᶜdhmw (ll. 3 and 4). Then, comes a change in the conjunctions: k- instead of l-. Had the k- pericope the same value as that of the l- phrases, there would not be any justification for a change of conjunction; e.g., the 4 purposes of ʾIlumquh's oracle in CIH 86 are each introduced by l- so that, viz lwfyhmw (l.4), wl/wśfhmw (ll. 5 and 6-7), and wl/gybhmw (ll. 8-9).

Furthermore, Beeston's translation of kstnḥṣn of RÉS 4176/6 is totally irrealistic. The whole pericope reads as follows:

> wlkd/bẓr/tʾlb/s(6)ʾr/ʾrwyn/bn/nśg/bn/mṣrn/kstnḥṣn/bnslm
> and that Taʾlab has prohibited the re(6)mainder of the mountain goats from being rounded up from the periphery because of the(ir) possibly becoming heavy with young.

The composite conjunction lkd is a redactional hinge introducing each main edict of the god Taʾlab. — nśg, cf. Ar nasaja "to draw together" in LaAEL, p. 2787B, and Sab hnśg to compose of RÉS 4708A (cf. JaMAR, XVI [1988], p. 61). — mṣr, cf. JaSIMB, p. 75A. —

nḥṣ, cf. Ar naḥaṣa "valde obesus fuit, de camela" and nâḥûṣ/naḥîṣ "valde obesus, de camela" (cf. FrLAL, IV, p. 251A). Beeston's translation of kstnḥṣn as "when they are pregnant" is unattainable because, if so, Taʾlab's devotees would have 1st to check the condition of each goat before taking any action, an unrealizable task to perform because the beasts would never give man the time even to start, beside the fact that for many months the beasts are not heavy/fat with young. Therefore, the god's interdict would be inapplicable at all time beside being without object for some months. Beeston's understanding of stnḥṣ as a perfect is disproved by his own translation as a present, "is," not "were, have been." This last translation would indeed be absurd in the text. Since stnḥṣ cannot be either perfect or imperfect, there is no choice but to take it as a masdar.

On the so-called apheresis of the initial consonant y- of the imperfect verbal forms (cf. above, p. 198), Beeston states that

> "Prefixing of the jussive/optative particle l [] to a verb occasionally entails graphic deletion of imperfect y-" (p. 15, par 5:8); my underlining.

and he claims that "the authenticity of the phenomenon is guaranteed by pl forms such as" (cf. l.c.) lhwfrnn of Ja 669/14-15 and lhdrnn of Ja 720/14. It should be pointed out that Beeston's approach is rather simplistic: it looks as if, seeing the -nn ending, he could think only of the form of his "-N imperfect," : "y-...-nn" (p. 15, par 5:7). But, the -nn ending is also the noun dual ending, as evidenced twice in Ja 716/2-3:

> -/bytNN/hrn/w(3)drḥn/ʾqwl/śᶜbNN/ḥśdm/wġymn.

The -nn ending basically is the article -n with a 2nd reinforcing -n; the dual value is imposed in the present case by the 2 apposed proper names.

Therefore, nothing prevents hdrnn of Ja 720/14 from receiving the same interpretation if hdr is understood as an infinitive, as it is in JaSIMB, p. 436B: "l"nn [inf.], 720/14;" cf. also M. Höfner: "hdrnn kann so, wie es dasteht, nur Infinitiv mit Abstraktendung -n + (verstärkender) n-Endung sein" (cf. Hebräische Wortforschung, Leiden, 1967, p. 111).

Note also that Beeston erroneously copies "wbn" instead of "wbnw" of Ja 735/14.

The preceding interpretation applied to lhwfrnn of Ja 669/14-15 is even reinforced by the context which, as RoPH 105 (cf. above, p.198), shows a change of conjunctions, viz F yhqnynn (ll. 11-12); wyhślnn (l. 13)

> w L hwfrnn (ll. 14-15); and
> w L ḥmdnn (l. 16).

For the last 2 verbal expressions, cf. already "l"nn [inf.], 669/14-15" and "l" [inf.] ... "nn, 669/16" in JaSIMB, respectively, pp. 432B and 436B. Furthermore, the antecedents of the 1st 2 infinitives are the 4 authors of the text. But, Beeston changes the

subjects of the 3rd infinitive to retain ʾttthmw/wbnhmw. What he does with the 4th verbal form remains unknown. At any rate, BiDO had already preceded Beeston by taking as subjects of the last 2 verbal forms "their wives and sons" (p.140). Such an opinion is unacceptable because it would be very strange indeed would the authors of a text not take the direct responsibility of even 1, less 2 of the actions which they promised to their god in order to have their wish fulfilled. Such a remark is even more obvious in the case of the 4th verbal form lḥmdnn since it is a question of thanking the god, viz according to BiDO, those authors would have left up to "their wives and sons" to thank their god for themselves!

It is in connection with Ja 669/14-16 that Jacques Ryckmans (cf. above, p. 195) presents as absolutely certain the lunatic hypothesis of the most fanatical pan-Ar of the laudemus invicem gang, E.A. al-Ghul, who would impose the NA rite of cutting a part of the hair to the expression lhwfrnn/ʾttthmw/wbnhmw because of the simple fact that Ar wafrat means "lock of hair, curl." This idiotic hypothesis is even summarized in a note of BiDO (p. 140). Quite unexpectedly and astonishingly indeed, al-Ghul's suggestion must have appeared too farfetched even to the other pan-Ar fanatics of SDDS, including Jacques Ryckmans, who took no heed of it (p. 157).

Even A.G. Lundin rejects SDDS's etymology of wfr of hwfr (cf. Le Muséon, 97 [1984], p. 178) and maintains the meaning of "to cultivate" for hwfr. He translates ywm/h(5)wfr/mwfrt/ʾl(6)lmqh of YM 1044/4-6 as "lorsqu'il cultivait les propriétés d'Almaqah." But, as already stated in JaSIMB, p. 175B: "Arabic wafara, 4th form 'to increase'," and mwfrt does not mean "propriétés" (= qny, pl. ʾqnyt; cf. JaSIMB, p. 447B); the word means "cultivated plots." Furthermore, the simple fact that the author of the text would "cultivate the estates" of the god is hardly important enough to be mentioned after "when he was a priest." Moreover, Lundin translates lh(15)wfrnn/ʾttthmw/wbnhmw(16)-ʿdy/mḫrmn of Ja 669/14-16) as "que sa femme et leurs enfants cultivent la terre pour le temple." This translation is unacceptable for 3 reasons.

(a) He erroneously transcribes "ʾttthw" ("sa femme") instead of ʾttthmw ("their wives").

(b) The direct complement "la terre" is Lundin's invention to give a meaning to his phrase.

(c) The preposition ʿdy is basically a local preposition, "in" (cf., e.g., JaSIMB, p. 444); it simply cannot be given the meaning of "pour."

Note that BiDO (p. 140) refers hwfr to "MüAf024/p. 151," which has only the photo of the antiquity YM 375 = 1044, and she even speaks of "uned insc MüAf024/p. 15 (photo)" 4 lines further down. The text was published in JaCMYE, pp. 156-169. But, BiDO's "Selective Bibliography" (pp. 551-561) ignores that publication of 224 pages and 21 plates!!!

And this voluntary rejection enables BiDO to maintain the meaning of hwfr as "to make a pilgrimage" which JaCMYE clearly rejects (pp. 157-158). Quite surprisingly, Beeston translates h(5)wfr/mwrt as "he duly performed the (...?...)" in CIASA, I, p. 225. This is certainly a most brilliant translation! Then, in SDDS (p. 157), Beeston has found his equilibrium again: he reverts to the "make a pilgrimage" translation. For more details, cf. JaMAR, XIV (1982), pp. 248-249, where the text is referred to as A, because there is another text, B, engraved on the upper side of the stone, never mentioned before and which is published on p. 249, with my tracing of this short text.

(9) The last development of the question comes from W.W. Müller who, quite unexpectedly, calls the past interpretations in question (cf. Jemen-Report, 17 [1986], part 1, p. 100). After noting that the meagre description of the text does not betray what the ceremony really was - an illustrious "lapalissade" - he states that "auch jene kurze Passage entzieht sich noch einem befriedigenden Verständnis (Z.9f)." Then, in an apparent contradiction with his general statement, he expresses his own view in the immediately following phrase as follows:

> "Auf jeden Fall dürften Frauen eine besondere Rolle gespielt haben, denn unter dem Geleit ihrer Begleiterinnen werden Beschwörerinnen aufgeboten, welche dem Gott Almaqah Pfänder gaben und während der Kulthandlung wohl auch etwas vergossen."

The preceding presentation practically summarizes Jacques Ryckmans' translation with the exception that Müller does not specify who his "Frauen" are or might be. But, his word "Frauen" necessarily excludes a major element of the text, viz the whole affair has as subjects kl/s̃ʿbn/sbʾ/wbnt/mrb. Here again, as in Jacques Rycmans' opinion, the elimination of kl/s̃ʿbn/sbʾ is a voluntary distortion of the text. His "Beschwörerinnen" is a gratuitous interpretation of rqt; and his 'unter dem Geleit ihrer Begleiterinnen" is the rendering of Beeston-Ryckmans' gratuitous interpretation of ʾntn/ʿtwfhn. Furthermore, Müller's "während ... vergossen" has no counterpart in the text and, therefore, is not intelligible. Finally, Müller's "auf jeden Fall" is a gross error because the following wording of his opinion is precisely what is debated, and not at all the minimum agreed upon by all.

(b) Robin examines the 2 examples of the 3rd feminine plural person mentioned by MüNAI (cf. above, p. 287).

(1) He first deals (pp. 175-176) with "CIH 45 + 44/8 ...]/k-l-brkn[..." (p. 175).

The case of CIH 44+.+45. - MüNAI's study of these 2 fragments (pp. 59-62) is most interesting. The basic question is the order of the 2 fragments.

On the one hand, although acknowledging that the sequence CIH 45+44 (it would be more

accurate to speak of .+CIH 45+.+44. because the 2 lateral sides of the central section of the original are missing) still remains uncertain, he nonetheless endorses Jacques Ryckmans' arrangement of CIH 45+44. After all, the 2 of them are members of the same *laudemus invicem gang*. On the other hand and without mentioning it at all, he corrects Jacques Ryckmans' scheme because the latter had ignored the initial m of CIH 45/6 which he reported at the end of 1.5, and had also wrongly considered nn (1.5) and nt (1.6) as the beginning of each line. A simple look at the photo of CIH 45 published by CIH shows that such a suggestion is manifestly wrong. But, MüNAI's correction of Jacques Ryckmans' work also involves (a) the replacement of the pericope brdʾ/wnqr/rḫmnn (CIH 44/3, attested in RÉS 4069/11) by his own unsupported concoction bnqr/wrdʾ/wmqm/rḫmnn, although brdʾ/wmqm is well-known (e.g., CIH 153/2), and

(b) the correct transcriptions of tm/wbḫl (CIH 45/7) instead of Jacques Ryckmans' doubly erroneous "(q)m wbḫ(y)l."

Furthermore, MüNAI repeats Jacques Ryckmans' error in CIH 44/5, viz "wbḫyl," which must be corrected to wbḫy[l], and he copies CIH's misreading of CIH 45/2, viz ...]t. But, he disregards CIH's correction in CIH, III, p. 319A: t instead of t, and]t is the end of s]t si]x. The photo makes it clear that each extremity of the letter ...]t of CIH 45/2 is curved inwardly; in the t letter (cf. t of tsn immediately below in 1.3), the extremities are straight, naturally. In addition, the exact difference between the letters ḳ and l has already been pointed out before (cf. above, p. 4). In the present case, that characteristic is present in each of the 4 examples of l as an upside-down segment the secant of which is at the level of the lower edge of the interlinear space.

Beside his gratuitous arrangement of CIH 45+.+44 although that involving CIH 44+.+45 is as feasible as the other, MüNAI also copied from Jacques Ryckmans the restoration of šrḥbʾl (1.5), which is based exclusively on Jacques Ryckmans' comparison with the fragmentary RÉS 4106. However, the recourse to RÉS 4106 remains unjustified not only because the palaeography of the 2 texts is greatly different but also because šrḥbʾl of RÉS 4106 is introduced by mr]ʿhw instead of mrʾhmw, as Jacques Ryckmans himself puts it. The restoration of mʿdkrb is quite plausible.

The presentation of MüNAI's transliteration of CIH 45 contains the 5 following errors to be added to that of 1.2 (cf. above).

1) klbrkn begins 1.8. This is impossible because the right edges of both the bottomless rectangle of the initial k and also of t of tm (1.7) are broken off and, therefore, that side cannot show even one millimeter of the original right side of the stone and all the more so in that the same right side would also have a rim similar to that at the bottom of 1.8.

2) The letter t of]tm (1.7) is placed above l of klbrkn. This is contradicted by the photo according to which the same t is above the initial k of klbrkn. Thus, the beginning of his 1.7 should read tm, without any dot on the left.

3) His alignment of ll. 5-6 upon 1.8 is therefore as baseless as his beginning of 1.8.

4) The beginning of his 1.6, "[wy]mnt" puts m above w of]tm/wbḫl of 1.7. In fact, that letter m is above the space between the 2 letters tm of 1.8. Even were klbrkn the beginning of 1.8 (MüNAI's opinion), there would not be enough space to put y at the beginning of 1.6; thus, the beginning of 1.6 in his own scheme should be mnt with wy reported at the end of 1.5.

5) The beginning of 1.5 should be [m]nn instead of "[rḫm]nn" as in his scheme because nn is just above nt of 1.6, and MüNAI pushed nn to the right in order to make room for the letters rḫ.

MüNAI did apparently not see that 2 passages of his commentary contradict each other because there is no cross-reference.

The 1st passage says that mflm is "wahrscheinlich der Eigenname einer Örtlichkeit oder Baulichkeit, weniger wahrscheinlich ein Terminus technicus bei Bewässerungsbauten"

followed by an etymological note (p. 60).

And then, on p. 62, he writes that "J. Ryckmans hat ... die Meinung vertreten, dass mflm kein Eigenname zu sein brauche, sondern ein Terminus technicus sein könnte."

MüNAI did not even see that the 2 passages deal with the very same question. Since then Jacques Ryckmans' grand idea became a certitude, and the final result is that SDDS (p. 44) does not register mflm under fll but simply add " → FLG" to guide the reader to their reading "mflg" with the following very convincing (!?) commentary, "{edd}," which means "remarks by present editors" (p. XVIII). In fact, thus, SDDS's authors — and Müller is one of them — correct mflm to "mflgm" and, as usual, they do not give either any justification or any reference to a study where any of those "present editors" would have explained their move. As usual, this is another case of arbitrary choice aimed at favoring some particular theory or whim retained by the dictatorial boss Beeston. It is evident, however, that, here again, those "present editors" did apparently not care to do their homework, viz to see whether or not their aprioristic reading was in fact supported by the photographic documentation, the most important precaution to be taken. Whether they checked it or not remains an open question. However, the reader does not forget that for those "present editors" and their followers the study of any photo would be useless because they repeatedly stated in the past that ḳ and l were undistinguishable in the late texts; therefore, it is quite logical for them not even to bother to check the photos because, in their opinion, these cannot a priori offer any solution. This is clear-

ly the antiscientifc approach of a haughty intellectual attitude creating a rule in order to suit the *pruritus of* an imagination to wander freely and, consequently, rejecting the objective testimony of the engraved text for the sake of their own whim. Here again that ugly aprioristic scheme is denounced once more because, here as in any other late text, there is a visible difference between the 2 letters ḫ and l. In CIH 540 and 541 the distinction between the 2 letters is that the upper left angle of the appendix of ḫ encroaches upon the lower edge of the linear space. The fact is easily seen when comparing ḫ and l, e.g., in CIH 540/11: Grbm/wLbtm, and especially in the best possible examples where the 2 letters follow each other, ʿGLm of CIH 540/14 and 16. Contrarily, the 2 l's of mflm are identical and the reading of the 2d as ḫ is erroneous. The reader will be edified to know that, after writing in 1976 (cf. his text above, p. 203), mflm is probably the proper name of a place or of a construction, less probably the the technical word of an irrigation building), the very same Müller publishes a note in ABADY, II (published in 1983, thus a full year after SDDS) stating simply that

> "MFLL ist sicherlich kein Eigenname; das Wort ist eher mflg zu lesen, was einen Ausflusskanal oder etwas Ähnliches bezeichnen dürfte" (p. 117, fn 86).

Of course, Müller does not inform his reader that such an opinion was already printed in SDDS, and especially that he copied his theory from Jacques Ryckmans AFTER having held the opposite!!! Müller's above-quoted statement is appended to U. Brunner's expression "Auslassmauer (MFLL?)" in l. 22. Nonetheless, the same author, U. Brunner, uses twice in the same page, in ll. 10 and 17, the expression "Kontrollmauer MFLL."

The only letters remaining of CIH 45/8 are ...]klbrkn[... Always on the lookout for something sensational, MüNAI claims that the expression is composed of the temporal composite conjunction k+l and "höchstwahrscheinlich" "bārakna, d.h. 'sie (pl. fem.) mögen segnen (oder preisen)" (p. 61),

an interpretation that was inspired by CIH, III, p. 319B. The most obvious objection is to figure out what or whom those women might or would bless, even were it possible to ascertain that there might be women involved in the text and that those women would be priestesses. In fact, on the one hand, nothing in the text indicates even remotely or indirectly that even a single woman is mentioned and, on the other hand, no text written even by a rš priest EVER alludes to any kind of blessing bestowed by him. Note that BiDO (p. 497) erroneously refers to Ja 122 under rš/wt; the text is Qat, and not Sab. MüNAI is looking for difficulties where there are none: klbrkn = kl[/]brkn every blessing; cf. the verb brk to bless, e.g., in Ja 1028/1 (cf. JaSHI, p. 42), always said of God. Finally, MüNAI lists 3 texts, Ja 735/9, 919/5 and RÉS 3980/5, where he claims to find the same feminine plural verbal form qtln. Only the 2nd reference is valid ... ever since 1963, the publication of my JaUT, p. 39:

> Ja 919/5: "šyʿn: -n, ending of the f. pl. attested here for the first time," and "šyʿn (f. pl.): 919/5" (p. 73A). And Müller's candle was not needed! For Ja 735/9, cf. above, pp. 185foll; for RÉS 3980/5, cf. below.

Back to Robin's study. - Robin invents a separator before the word. That word divider does not exist in CIH's photo or in pl.3 of MüNAI, and it could not exist as already explained above, p. 202, bottom. Moreover, Robin's invention is also grossly erroneous because it excludes without the slightest proof or indication that the initial k could not possibly be preceded by either w and, or anything else. Furthermore, he interprets that word as a verbal form ... also quite gratuitously. His explanation reads as follows: "la préformante y-, en contact avec la subjonction, se serait vocalisée et aurait disparu de la graphie" (p.176); this is COPIED from BeSG; cf. p.199. The very 1st question to be asked: what proof does he have to offer? N o n e. Furthermore, his statement is nothing but the presentation of his wholly hypothetical thesis; his statement does not prove anything except of voicing his own wishful mirage. And that thesis is already injected in his transcription of the text

not only by the invention of an inexistent separator,

but also by his idiotic and insane use of dashes in his transcriptions of texts which does nothing but create extra difficulties for the professionals who have to constantly forget them, and also inevitably leads the reader into error. As already stated above (p.204), the obvious solution is much simpler, viz kl brkn every blessing. After that, Robin invokes in support of his theory the comparison with "l-hwfrnn et l-ḥmdnn de Ja 69/14-15 et 16 où la finale -nn implique de reconnaître les inaccomplis l-(y)hwfrnn et l-(y)ḥmdnn" (p. 176).

The same reasoning is found AGAIN in BeSG (p.15, par 5:18); cf. above, p. 199.

(2) The 2nd example of the 3rd feminine person mentioned by MüNAI (p.62) is RÉS 3980/5: ...]fnb(?)n/wtf[... according to G. Garbini in AION, 20 (1970), p.403. Müller's basic error is to consider as certain Garbini's doubtful reading. The text is dealt with by Robin in p. 176 of his paper, of which fn 3 refers to his RoHT, II, "à paraître." It is found on p. 111 and photo in pl. 68c; this section of RoHT is not to be found in RoPH: the text is not listed on p. 622.

Robin reads l.5 as follows: ...w]tf^{n} bn wtf." The photo of pl. 68c is one of the too many very poor ones which do not allow the reader to see what Robin claims to have read. If /wtf/ can be read in the 2nd half of the line, the beginning is far from clear. The bottom of all letters is broken off; thus, ⌞⌟w⌞tf/⌟. The 1st half is quite different.

(a) The stone to the right of the initial ⌞f⌟ is damaged and the left edge of the section is seen as a large curved line; thus the letter "ṯ" of Robin is excluded.

(b) Between the 2 ⌞n⌟'s, there are 3 vertical lines and the spaces between the upper extremities of those 3 staffs cannot confirm Robin's reading of "ṯ" rather than "/b." A clear photo is needed to ascertain the reading: until it is published, the text can-

not be utilized in spite of Robin's testimony of "on lit sans aucune hésitation" (p. 176).

6 - qtly = "feminine plural" (cf. Mas-Gellas, l.c., pp. 176-180).

Arrived at the end of his treatment of Sab, C. Robin utters his victory cry by stating that the 3rd feminine plural of the Sab perfect, qtly, a form that "n'avait pas encore été reconnue" (Phoo!) is attested in no less than 4 texts, viz CIH 330/3, 581/3-4, Ga 92/4 and Ir 34/2. Note that the reference to Ga 92/4 is missing from Robin's announcement of his discovery in Raydān, 3 (1980), p. 190; cf. above, p. 177.

The situation, however, is different in BeSG (p. 14, par 5:4), who affirms that "until recently, the only recorded examples of fem pl in -Y were C 330/3, C 581/4." But, none of the 3 following publications refers to that verbal form; viz BeDGSA (1962 p. 23, par 21:2 ; HGASG (1943), p. 69; and G.M. Bauer's Yezik uzhnoaraviskoe pismennosti (Moscow), pp. 69-70. Furthermore, SDDS does not even list the form rtdy of CIH 330/3 p. 119), but mentions "d. [= dual] hqnyy" of CIH 581/3-4 (h[q]nyy) (p. 106). But, BeSG adds this: "but C. Robin notes that it is found also in" Ir 34/2 and Ga 92/4. How is it possible that Beeston abstains from voicing his opinion? Cf. above, p. 197, ((a)).

N o n e of the 4 examples (102 lines: the longest section) is v a l i d.

a - Robin's 1st example is CIH 330:

1 n^{c}mwd/wn^{c}mgd/wbn[t... /bnt/^{2}lt/.]
2 bn^{2}l/yhṣbḥ/^{2}mt[/... .../ḥ]
3 rthn/tġl/wrṯdy/m[...
4 t^{2}lb/rymm/w^{2}b^{c}l/[...

In flagrant contradiction with Robin's self-flattery CIH, I (1889), p. 369B, states this: "rtdy tertia persona feminini pluralis, non masculini dualis." Such an affirmation could hardly be clearer. Not satisfied with his initial blunder, Robin continues in his arrogant rut by writing 2 more erroneous statements.

(1) ^{2}mt means "servante(s)," thus, both singular and plural. Would Robin have paid more attention to CIH, he would have found "^{2}mh, pluralis voice ^{2}mt" (II [1914], pp. 129A, 327B and 575B;) cf. also III (1925), p. 52A: "^{2}mh 'ancillae'." He could also have read with great profit C. Conti Rossini's Chrestomathia arabica meridionalis epigraphica (Rome, 1931): "^{2}mt, pl. ^{2}mh" (p. 107A). Thus, ^{2}mt of 1.2 refers only to the 1st feminine name of 1.1, n^{c}mwd. Furthermore, ^{2}mt does not means, as Robin claims, "cliente," but maidservant. Strangely enough, Robin blindly endorses A.F.L. Beeston + the laudemus invicem gang + SDDS (p. 5) who anachronistically inject the middle-aged situation of servitude and bondage to pre-Islamic SA! Cf. already above, pp. 57, 67 and 79.

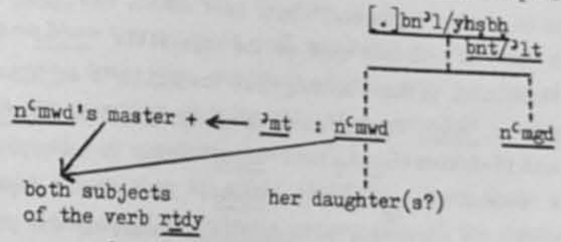
(2) He also erroneously admits the possibility that bn... (1.1) could be

"fil[s]." Since the 1st 2 names are feminine, it is necessary to expect bn[t daughter(s?).

(3) He erroneously considers bn^{2}l (1.2) as complete; it could also be [.]bn^{2}l (ll. 1-2).

(4) He fails to take the missing name determining ^{2}mt (1.2) into consideration because it almost certainly is a masculine personal name.

Therefore, rṯdy is a dual having as antecedents both n^{c}mwd (1.1), the 1st and main subject of the text, and the man who was listed in 1.2. Although the [ḫ]rt belongs to the 2 women and her [= n^{c}mwd's] daughter(s?), the master of the 1st of them, n^{c}mwd, is the main subject of the activity described by rtdy "the 2 of them have entrusted to the care of." The following diagram illustrates the structural composition of the text:



b - Robin's 2nd example (pp. 177-178) is CIH 581. JaMaR, XII (1982), pp. 17-28, studied it and rebutted A.F.L. Beeston's theories (cf. Arabian Studies, 4 [1978], pp. 21-25), which are endorsed by Robin (cf. fn 1 of p. 177) who is not honest enough to acknowledge my 1982 study of the text (cf. above). Here are a few additional notes. Robin erroneously considers "bnt/^{2}lt" (1.1) as certain; it is a restoration based on 1.2. Furthermore, his argumentation contains an obvious contradiction. On the one hand, he writes that "j'incline à penser qu'il y a seulement deux dédicantes" as he considers "[h]n^{2}t[w]n" of ll. 1-2 as "un second gentilice." On the other hand, 4 lines below, "les auteurs du texte sont des femmes au nombre de trois." But, where is the 3rd woman? There is none, since ll. 1-2 contain only 3 names, n^{c}mgd, [h]n^{2}t[w]n and n^{c}mt, which are susceptible of being feminine, and, as pointed out above, the 2nd is understood as a gentilic name. Finally, ^{2}mh is translated as "clientes," as ^{2}mt of CIH 330/3 was. Thus, there cannot be any lapsus calami; it is the proof of the confusion of the singular ^{2}mt and its plural ^{2}mh. And, it must be restated again that "^{2}mt does not mean cliente" or "female vassal" by Beeston (cf. l.c., p. 81), but "maidservant." Summarily, both Robin's and Beeston's infatuation with social medieval institutions is as overwhelming and as blinding as that with pan-classical Ar fanaticism (cf. already, p. 206).

c - Robin's 3rd example (p. 178) is Ga 92 (cf. G. Garbini in AION, 36 [1976], pp. 301-302 and pl. 3b). Robin's thesis cannot be proven by this fragmentary inscription

is based exclusively on his betrayal of the inscription as he voluntarily chose not even to mention that the missing central section (ll. 2-3) contained several masculine names introduced by bmqm by the power of (l. 2), viz "...(3)gdym/ʾḥgn/wmrtdm/...(4) wḫtfn/; these names are followed immediately by ngrnyn the Nagrânite, which in turn immediately precedes rtdy. There could thus be a change of subjects of the last rtdy. In addition, Robin gratuitously translated bnthw (l.1) as "ses filles" without even giving the slightest proof that there were even 2. Cf. above, pp. 206-207, CIH 230, for a parallel construction with ʾmt instead of bmqm.

d - The 4th example used by Robin (pp. 178-179) is Ir 34 (cf. M.A. al-Iryani, Fî taʾrîḫ al-yaman, Cairo, 1973, pp. 174-175). As Robin states that the text is known only by "une copie peu fidèle," it is difficult to understand how he could possibly justify any solution on a text "douteux." But, here again, his study is very amateurish. He transcribes the 2nd feminine name as the impossible "ʾyyšf" (l.1), which is the reading of the facsimile, without noting that the editor's Ar transliteration has the correct spelling, viz ʾbyšf. Furthermore, the verb in l.2 is "hqnyy." in the facsimile, but the Ar transliteration has مقنّي = "mqyny" which is incomprehensible and certainly is a misprint instead of مقنّي = "hqnty." This verbal form, which is restored by the editor, should be corrected to hqny(t)[w] because of the plural ʾmh (l.2) and the plural pronoun -hn attested 4 times in the text. The editor miscopied t of hqnytw as y, and was unable to read the final w. For Jacques Ryckmans and Ir 34, cf. already JaCMYE, pp. 275-276.

B - Min (cf. Mas-Gellas, l.c., pp. 180-181).

In his treatment of Min, Robin abandons the RÉS sigla in favor of the numerotation of M. Capuzzi, Iscrizioni minee (Naples, 1974). Such an initiative is unjustified because there are too many errors in Capuzzi's book. E.g., in the 1st mentioned inscription, "M 203" = RÉS 2980bis (cf. Capuzzi, pp. 59-60), correct "J. Ryckmans" (p. 59) to "G. Ryckmans;" the author claims to follow C. Conti Rossini, Chrestomathia ..., no. 74, pp. 81-82 , but in l.4, Conti Rossini's "q" is transformed to "w;" and bn/kl(8)ynkrsm becomes "bn/kl...(8)ynkrs,m," but E. Glaser (Altjemenische Studien, I, Leipzig, 1923, p. 33) has "bnkl(8)[d]ynkrsm." Furthermore, Conti Rossini refers only to the above-listed publication of Glaser. Yet, he eliminates all diacritical marks of Glaser's text; e.g., "sᶜ[d]b" and "[d]hbl" of l.2. For "M," cf; above, pp. 73 and 92-93.

1 - gtl = both "dual and plural masculine" (cf. Mas-Gellas, l.c., pp. 180-181).

Robin starts with the verbal form gtl which he claims to be both dual and plural, and each hypothesis is exemplified by 2 texts. In each of the 5 inscriptions,

viz RÉS 2980bis, 3022, 3021, 2965 and 2976, the singular verb always has as subject the 1st author of the text, who represents the natural or official authority that performs and bears the responsibility of ślʾ, that is neither an apocopated dual or plural.

a - In RÉS 2980bis, the actual author is a father who also lists his son at the beginning of the inscription; cf. also the 2 singular verbs ᶜlly and sᶜ(d)b (l.2) with the same unique author, the father who is responsible for those 2 activities. Robin concludes his study of the text as follows:

> "Il ne faut pas en[= similar graph of both singular and plural] conclure que le duel n'existe pas en mineen: dans la mesure où les pronoms et les substantifs possèdent une forme duel distincte du singulier et du pluriel, il semble vraisemblable qu'il en était de même pour le verbe, même si cela n'apparaît pas dans la graphie consonantique" (p. 180).

But, it is Robin alone who interjects the idea of dual/plural in those forms, and he does apparently not see that the usage of dual or plural in the nouns and pronouns (-s and -sww: singular; -smn: dual; -sm: masculine plural, and -sn: feminine plural) has nothing to do with the question of authorship of a text. Of course, when referring, e.g., to the persons of his coauthor(s), the main author has to use the dual or the plural for the simple reason that the other coauthors have their own bodies, a fact that he cannot ignore although he is the sole responsible of the text (cf. already above, pp. 179 and 183). Therefore, the 2 situations are manifestly different, and the 2nd cannot have any bearing on the 1st. But, that apparently is... too much for Robin.

b - In RÉS 3022, the real author of ślʾ (l.1), ktrb (l.1) and sbrr (l.2) is the 1st of kbry the 2 leaders listed in l.1. In l.1 also, instead of Robin's copy of "ᶜlⁿ," which he picked up from H.Y. Nâmî (cf. Majallat kulliyat al-ādâb [Cairo], 16 [1954], p. 13), the reading of wlg (cf. already RÉS, V [1929], p. 305) is demanded by the Glaser squeeze A 195.

c - In RÉS 3021, the real author of bny is the 1st of the 3 persons listed in l.1, the last 2 ones being most probably the 1st's kid brothers. Robin's copy of "bny" preceding ʾldrʾ must be corrected to bhny on the basis of the Glaser squeeze A 200a.

d - In RÉS 2965, the actual author of bny (l.1) and frᶜ (l.2) is the 1st person listed in l.1, the 2 others being the 1st's sons; cf. also the singular verb in rtd/yklʾl/ wbhnsw (l.3), and each of the 5 direct complements determined by the plural personal pronoun -sm (e.g., ʾnfssm of l.3).

The 4 most important differences with the texts of either RÉS or Capuzzi (p. 54) based on the squeezes A 258a-g = Gl 1236+1237 = RÉS 2965 are as follows:

(a) L.3 contains the name of a new Min king and his royal title, viz. ʾbkrb/ṣdq/mlkh/ mᶜnm ʾAbkarib Ṣâdiq, king of Maᶜînum.

(b) The last 3 words of the inscription located at the end of l.3 ever since J. Halévy (cf. JA, 19 [1872], p. 222, no. 465) and F. Hommel (Süd-arabische Chrestomathie, Munich, 1893, pp. 104-105), are actually engraved at the very end of l.4. The letters of those words are shorter than all the others, viz. 4 cm against 8 cm; the last m is 2.5 cm away from the end of the lines, the width of the last 3 words is 33.5 cm; they follow -/wghftnhn of l.3 after a gap of 47 cm.

(c) Toward the beginning of l.4, read ʾślʾ offerings instead of śʾlʾ.

(d) For the number 𐩻𐩿𐩺𐩻𐩽 (l.4) = 180, cf. my lengthy 1989 review and reedition of the texts published by A. de Maigret et al., The Sabaean Archaeological Complex in the Wādī Yalā ..., in JaMAR, XVII (1989), pp. 81-82.

2 - Of the normal masculine plural form qtlw Robin (cf. Mas-Gellas, l.c., p. 181) refers to the example of RÉS 3016, and his reading was "vérifiée sur photographie" (p. 181). Here again, Robin thinks that his photo gives the authentic contents of the inscription and does not at all take into consideration that E. Glaser brought back 2 squeezes of the text, A 287 a and b, which show a more complete text than his (unpublished!?!?) photo; viz the beginning of l.1 reads ⌜⌝wywm instead of RÉS's "y]wm" copied by Robin. Thus, this is the very same fundamental mistake as that of J.F. Healey's in his dealing with Nabataean JsaN 17 (cf. already above, pp. 108-110). It should be pointed out that the difference between ġ and l is rather unique in that it is not the position of the upper appendix but the light bending inwardly of the top of the vertical staff in ġ. Finally, Robin inaccurately translates qnyw/wbrg/wqyḍ (l.1) as "ils acquirent, payèrent et réglèrent." The verbal series means "they possessed and acquired and prepared."

On p. 181, Robin also refers to "sqnyw" of RÉS 2948 A/4, as in Capuzzi, p. 49, no. 184A, stating that it is "une erreur de restitution." But, this error comes from A.F.L. Beeston in Le Muséon, 66 (1953), p. 118, not from Capuzzi since she clearly states: "Lettura: Beeston, Lexicography V." Robin claims that "on lit sur la pierre r]šw^{n} ʾqn (ou šqn?)." Here again, Robin thinks that nobody saw the text before him, and he ignores H.Y. Nāmī's reading: wšᶜn/ʾqn[y] (cf. Majallat kulliyat al-ādāb, 19 [Dec., 1957, p. 113], where wšᶜn could be either a noun or an infinitive.

e - For RÉS 2976, cf. below, p. 237.

C - Qat (cf. Mas-Gellas, l.c., pp. 181-182).

Robin examines the 2 verbal forms qtlw and qtln.

1 - qtlw = "masculine plural and dual."

The author states that qtlw is masculine both plural and dual because each form could have had its own particular vocalization (p. 182). If it is impossible to argue with such a statement in unvocalized texts, it is equally impossible for Robin to prove

he is right. Here again, his thesis is annihilated by his own admission that such texts manifest the tendency of replacing the dual by the plural and, therefore, that they "n'attestent pas réellement une forme particulière de duel" (cf. already above, p. 184). If such is the case, Robin does not have any ground, solid or not, to stand on. Robin gives 3 examples of his dual qtlw.

a - In Ja 340, a man and his wife use the dual in pronouns, dw (l.1) and -s[m]y (l.3), but the plural in the verbs sqnyw (l.1) and rtdw (l.3).

b - BaRoNAH (cf. Raydān, 2 [1979], pp. 11-22 and photo in p. 12, in the Ar section) is the 2nd text chosen by Robin. The inscription reads as follows:

1	mšᶜrm/wḫṯyn/dw	The whole text is here reproduced because it - and it alone - best shows how inconsiderate and totally abnormal is the 2 editors' (viz. M.A. Bāfaqīh and C. Robin) interpretation of mg (l.2) as a temporal conjunction which destroys a well-balanced, complete dedicatory inscription and, instead, let the 2 coauthors hanging down in the air without any principal sentence ..... and, apparently, the 2 editors did not even realise how irrational their deliberate choice actually is!!!
2	hgbb/wqyln/mg/hᶜ	
3	šqw/lʾlsmy/wmrʾam	
4	y/ᶜm/dᶜdbtm/bᶜl/n	
5	ᶜmn/bḫtn/lqfysmy	
6	wwfy/bklsmy/wʾby	
7	tsmy/wkl/qnyw	

(1) The inscription presents the same situation as Ja 340, viz a man and his wife use the dual in pronouns, dw (l.1) and -smy (ll. 3, 3-4, 5, 6 and 7), but the plural in the verbs hᶜšqw (ll.2-3) and qnyw (l.7).

(2) The 2 coeditors erroneously translate the dual dw as "بغوي" because d does not introduce a blood-relationship. - Regardless of their Ar counterparts, both hgbb and qyln cannot be translated as اصبح and الفيل, respectively (p. 14).

(3) The transliteration (p.12) in Ar letters, "ﻣﻞ" becomes "ml" in the French transcription which, thus, notifies the reader that the doubt indicated by the question mark either vanished or did not exist. But it reappears in the commentary (p. 16): "ﻣﻞ." The coeditors state that the difference between ġ and l is hard to see (cf. above, p. 204). They saw no difference between that letter and the 7 l's of the text(!); "il n'y a pas de pire aveugle que celui qui ne veut pas voir." Yet, there is a difference; viz the upper side of the appendix is only slightly concave in ġ of mg; it is strongly concave in all l's. Then, the coeditors embark on a flimpsy argumentation lacking in consistency and rationality, aiming only at proving that they had made up their minds on the word being some kind of temporal conjunction. Their initial reaction was, and rightly so, to consider their "ml?" as the 2nd element of a tribe name on the basis of hwln/hr of RÉS 4274/1. But, they immediately abandoned the idea and ended up suggesting without giving even the flimsiest reason that their "ml?" had the meaning of ywm, and they chose "ml" instead of "mg" because mtll means "maḍā (i), makata" ("to pass, go away," and "to stop, tarry") in the texts of al-ᶜUqlah as well as in Ingrams 1

(cf. A.J. Drewes, in BiOr, 11 [1954], pp. 93A-94A). But, what can their "ml?" have in common with the Ḥaḍr verb mtll? Then, they jump to Daṯīnah "millá," which LeGD (p.2712) describes as "préposition particulière au dialecte daṯīnois, 'près, à côté de'" (the underlining is mine). And, passing through the conjunction matá (cf. mty in Min and Qat + imperfect), they end up giving their "ml?" the meaning of "ᶜindamā = when,as." The best parallel of the preceding demented acrobatics consisting of jumping from B to E, then to M, then to R, to end up with the preconceived idea of what textual Z should mean is J.Ryckmans and Co.'s idiotic aerial gymnastics on ᶜrm,a 2nd Qat personal name (cf. JaMAB, XVI [1988], pp. 92-102). Finally, Robin obviously repeats the coeditors's translation of ᶜindamā ᵓanjazā as "quand ils ont réalisé (?)." Therefore, the Ar text proves that the coeditors understood hᶜšqw as a masculine dual, a feature not evident in the French translation. The place of "(?)" proves that the meaning of "ml?" is certain. In addition, the editors note that hᶜšq = sᶜšq, but fail to point out the Sab influence on the authors of the text; it is the 1st occurrence of the Sab form hᶜšq in a Qat text. Furthermore, the coeditors refer to 3 texts with sᶜšq, and the last one is "J 470/2-3." The number must be corrected to "2470." Finally, the name of the 2nd clan is double, viz qyln/mg Qaylān Maggi; mg is known as a personal name in both Saf (e.g., CIS 4269) and in Tham (e.g., Hu 22b).

(4) The coeditors' translation of bklsmy (1.6) as المقيمين على ارضهما (p.16) belongs to a commentary, not to a translation. It is also erroneous because there is no question of the coeditors' ᵓrḍ in the text. The authors' estates are mentioned in ᵓbytsmy, which immediately follows bklsmy, and this implies that those bkl did not live in the authors' houses or on their properties. The pericope simply means the attendants of them both and the houses of them both. Furthermore, the coeditors's commentary (p. 16) reproduces VL 9/1 in 2 lines, erroneously miscopies "ذو روين" instead of drwyn, and translates the end of the line mᶜrm/bkl/tmnᶜ as "معنم مقيمون بتومنح". Their preposition bi introducing tmnᶜ does not exist in the text, and kl is eliminated. Furthermore, the coeditors fail to refer to JaSIMB, p. 187B, which justifies translating the pericope as "Maᶜīn in all Timnaᶜ." The coeditors's translation of mᶜrm/bkl/tmnᶜ makes it certain that they consider that pericope as the 2nd subject of bnyw, the 1st being ᵓwsᵓl/ - - /kbr/mᶜrm/btmnᶜ (1.1). Such also was the opinion of the editor of the inscription, M.A. al-Ghul, in BSOAS, 22 (1959), pp. 419-420, where, on the basis of CIH 601/13: lḫwr/wbkl/bhgrn/ṣrwḫ, he argues that VL 9 "denotes the Minaeans who were full or permanent residents in Timnāᶜ and who would own property of some kind there" (p. 420). Such an argumentation is out of place because the text does not mention the noun/verb ḫwr, and nobody has the right to restore it for the sake of his own preconceived idea. Furthermore, to restrict the bkl to "own property of some kind" is to repeat the thesis

as a way to prove it. J. Ryckmans further distorts the normal understanding of bkl (cf. BiOr, 24 [1967], pp. 271B-272B). Even his 1st statement, viz "les bkl avaient des obligations envers les propriétaires du terrain, en l'occurrence l'Etat (CIH 601, 13 sqq)" (p. 271B),

is erroneous. Jacques Ryckmans distorts completely CIH 601/13 which is reproduced above; he eliminated ḫwr from his reasoning, and his biased argumentation lead him to state that "le bkl pourrait donc présenter une certaine analogie avec le muškenum babylonien" (p. 272A).

Obviously enough, nobody knows what the so-called "une certaine analogie" actually means or might eventually mean, and the reader is left to think what he wishes. But, muškēnum means "Palasthöriger, Armer" (cf. W. von Soden, Akkadisches Handwörterbuch, Wiesbaden, 1972, II, p. 684A). The reader cannot but wonder what the analogy might be between a dependant (cf. JaCMYE, pp. 96-97), a palace slave/an almoner, and the Minaean foreigners in Timnaᶜ. Another example of the confuse, confusing mind for which J. Ryckmans is famous for; cf. above, p. 192.

Furthermore, the fundamental objection against al-Ghul's interpretation is as follows: as mᶜrm/btmnᶜ are represented and led by their kbr, why are mᶜrm/bkl/tmnᶜ not represented and led by their own kbr in a community enterprise which needed a lot of planning since it consisted of building a large mausoleum lᵓᶜbsm/mᶜrm for their tribe Maᶜīnum? If the mᶜrm/bkl/mᶜrm had their leader as they should since every community has one, why is he not mentioned explicitely? These 2 questions are adequately answered if kbr is determined by both mᶜrm/ - /wmᶜrm/ - - . Thus, the 2 sections of mᶜrm residing in Timnaᶜ are those of Timnaᶜ proper and those of the greater Timnaᶜ area. The plural bnyw is in accord with the actual builders of the mausoleum, ᵓwsᵓl and all the Minaeans in and around Timnaᶜ. The whole pericope of VL 9/1-2

ᵓwsᵓl/ - - /kbr/mᶜrm/btmnᶜ/wmᶜrm/bkl/tm(2)nᶜ may be translated as

ᵓAwsᵓil - - , leader of both Maᶜīnum in Timnaᶜ and haᶜīnum in all [= the greater] Tim(2)naᶜ.

c - The same characteristics of both Ja 540 and BaRoNAH are also attested in Van Beek 1 with an extra touch of variety. A man and his wife use the plural in bnw (1.1) and the 2 verbs brᵓw (1.1) and rtdw (1.5), but the pronouns are either dual (ll. 2, 3, twice, 4 and 5) or singular masculine -sww (1.6) if need be. Furthermore, Robin confuses g and l HERE AGAIN in Van Beek 1(cf. Mas-Gellas, l.c., p. 182) in his "Gwd^{m}" of 1.1. Yet, as usual, the difference is so obvious in 1.4: ygl where the 2 letters are contiguous to each other and, therefore, offer the best possible opportunity to study them (cf. other examples above, pp. 4, 202-204, 210 and 211). The left side of the appendix is a concave line in each case, but that concave line is vertical in g, and as-

lant toward the staff itself in l, exactly as in CIH 850. Robin's "gwdm" which has no entry in AvGIAS, II (1980), pp. 263B-264A, must be corrected to lwdm; cf. Ar lawida " لوى II lawwad ... s'écarter du chemin (عن)" (cf. C. Denizeau, Dictionnaire des parlers arabes de Syria, Liban et Palestine, Paris, 1960, p. 483).

2 - qtln = "feminine plural" (cf. Mas-Gellas, l.c., p. 182).

The 2 texts AM 757 and 208+177 are listed by Robin (p. 182) as illustrating the feminine plural form qtln, as in Ḥaḍr (cf. below). The verbal form was duly identified by their editor, J. Pirenne, in CIASA, I, p. 150.

a = AM 208+177 (cf. J. Pirenne in CIASA, I, pp. 179-182). On p. 179, Pirenne's name and the date of her writing are missing. - All my notes are based on my reading of the stones and my squeezes made in January 1950. Cf. addendum of p. 230, below.

The letter f is composed of 2 separate vertical sections the extremities of which sometimes are either distinctly separated from each other (e.g., fqḍtn of 1.3), contiguous (e.g., šft of 1.6), or fused into a single line (e.g., wfy of 1.6).

The last letter of both l.1, ʾ, and of l.2, t, must be put between brackets.

L.5: the ġ looks like a rectangular ɣ with a short bar appended to the extremity of the crossbar (for a study of ġ, cf. JaMAR, XIV [1982], p. 300); = ٦.

Ll. 5-6: at the beginning of l.6 there was still on the stone the upper 3rd of r. Since the last 2 letters of l.5 are destroyed, the word is m..ʿrj. Robin had suggested the restoration of "m[tb]" to the editor (p. 148, commentary on AM 757). This restoration - based on the writer's prurient urge to talk - was still 1 letter too short, and mtb does not even exist in Sab, but could at best be the plural of mtbt reward, rescript (cf. JaSIMB, p. 441B). Furthermore, in his paper analyzed here (p. 182), Robin repudiates his suggestion and now supposes the existence of 3 missing letters, "m[..."

L. 6: the ʾ of mrʾ was left incomplete: the 2 short strokes immediately above the bottomless rectangle were not engraved: ' ⊓

L.7: the last word reads s[bʾ].

L.8: the beginning reads [ʿd]/.

L.9: the 1st letter reads (ḍ).

L.10: the last letter reads (n).

L.11: the last letter is (b).

L.12: the 1st letter is (ṯ).

b = AM 757 (cf. J. Pirenne in CIASA, I, pp. 147-157). - No photo of the left-hand side with ll. 17-18 is reproduced.

L.2: the last letter reads (t), and there is no need to restore a separator at the end.

L.3: the last letter is (m), only the 2 horizontal stokes are missing.

L.4 reads rʾsʿn/ɹ[ʿ]m/d(d)[w]nm/ʿd/mɹ[ḫ].

L.5: the end is [/dġ].

L.6: the end is mɹ[..r].

L.7: the last word reads mrʾ(s)[n].

L.9: suppress the last separator.

L.10: the last letter is (z). L.16: the initial letter is restored, [t].

The following notes are in order.

(1) Robin inaccurately speaks of AM 208+177 as " le texte parallèle" of AM 757. Pirenne had already spoken of "ce parallélisme des textes" (p. 148), but she describes it as follows: "les deux documents ... sont rédigés dans les mêmes termes, sauf pour le nom du dieu et de son temple" (p. 147).

Pirenne, however, overlooked the only textual variant : bmḫrms of the 1st (l.4) and ʿd/ḳm][p]rms of the 2nd (11.4-5). - The temples of both wrfw of the 1st and of ʿm of the 2nd are found in the same city, dġylm. As pointed out by Pirenne, this detail calls to mind RÉS 3688/11 (p. 182): bbyt/wrfw/wʿm/dlbḫ/bdġylm. But, šbʿn, the temple of wrfw (AM 208+177/4) and ḫṯbm, the temple of ʿm/dġylm (AM 757/4-5) are both bhgrn/dġylm. Therefore, the word byt of RÉS 3688/11 must be understood as a dual in defective writing.

(2) Robin's superficial analysis distorts the morphology of the text by referring only to the 3rd personal feminine plural qtln in the 2 verbal forms sqnyn and šftn, respectively, in ll. 3, 7 or 6. Other words are equally important and cannot be dismissed in favor of a partial and prejudiced thesis. Robin renders bn of bnsmy/ʾbkrb/wnʿmgd (ll. 1-2 of each text) as "enfants," which is (ʾ)wld. The translation is unjustified because neither ʾbkrb nor nʿmgd can be proven to be feminine in the present text; for nʿmgd both masculine (cf. Ḥaḍr BeEAG 1/1) and feminine (e.g., CIH 581/1 and DJE 22/1), and the gender of nʿmt of Fa 3/4 remains unknown because the name is the 3rd in a series of 8 names introduced by wld children. Thus, bn must be translated as "sons of," as Pirenne did (p. 148; no remark on p. 149). The preceding interpretation is confirmed by the masculine plural dtw (1.2; cf., e.g., Ja 119/1, 343 A/2 and B/2, 2457/1 and AM 335/1) which is simply transliterated by Robin without any comment of any kind. The 2 verbs sqnyn and šftn are feminine plural, having each 2 women as antecedent. These 2 forms exclude the 2 bnsmy/ʾbkrb/wnʿmgd from being also subjects; otherwise, the text would have sqnyw and šftw (or šft because of the annexed pronoun -s), respectively, 2 other cases of a plural verbal forms used instead of the dual. The plural personal -sn, attested 5 times in the text, follows the pattern of the 2 preceding verbal forms.

(3) The expression lwfy (AM 208+177/6, 757/7) is translated differently in 1.7 (cf. also 1.6) "pour le succès," but in 1.12 (cf. also 1.11) "pour le salut" by Pirenne (pp. 148 and 182). The 1st rendering is copied from Beeston's translation (p. 153), who is apparently unable to see the basic difference between the 2 meanings "safety" and "success" (cf. SDDS, p. 158). Even in the hypothesis of his understanding of sbʾ (cf. below), the 1st meaning "safety," which should be maintained, does not necessarily imply "success," regardless whether the trip was successful or not. Of course, it was successful otherwise that trip would have never been mentioned at all. But, the women spo-

ke of "safety" as by far the best outcome of the royal trip.

(4) The word ḏbḥ (cf. already JaMAB, XVI [1988], pp. 35-36) is erroneously translated by Pirenne as "or" (pp. 148 and 182). She adopts 2 different translations in CIASA, either "or" in I, e.g., pp. 131, 141, etc., and II, pp. 141 and 152, and "'or'" in II, pp. 148, 150, etc. What is the difference between "or" and "'or'"? Who knows?

Beeston also adopts 2 different translations in CIASA, viz

"bronze" in II, e.g., pp. 6, 9, 13, etc., but

"golden" in both I, e.g., pp 42, 47, 72, etc., and II, p. 68.

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E.g., "96.21/s9/064 B" 2 (cf. Tables, p. 59) was published as

"064/s9/96.21 n° 2" (cf. I, p. 283; and

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The following corrections should be reported in Tables, p.60 (the only page involved in the preceding remarks):(a) extreme right column: l.4: read "04" instead of "03;"
l.5: read "06" instead of "05;"
l.29: read "4-5" instead of "04;"
l.41: read "2-3" instead of "02.;"

(b) 2nd column from the right: l.18 should be reported below l. 21.

(5) The most important feature is rtdw (AM 208+177/13 and 757/14). This verb, which obviously reminds of dtw, indicates that, although bnsmy did not apparently have anything to do with the 2 activities of sqny and šft, they nonetheless participate in the act of rtdw. This information is unusual because the subject of rtd normally is the same as that of sqny.

(6) The historical importance of AM 757 is the expression

byw(9)n/bs/sb³/rḥbtn/ᶜbr(10)mlk/sb³/w³rḍ/ḥmyrm (ll. 8-10)

(also in AM 208+177/7-9) which is translated by Pirenne as follows, on the basis of Beeston's commentary (cf. below):

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which becomes "au jour où [il s'est rendu à] Ruḥabatan auprès du roi de Saba³ et au pays de Ḥimyar" (p. 182);

thus, "Ruḥabatan" and "Raḥabatan;" the comma "," after the preceding name and none; and

"à la terre" and "au pays." Beeston sent Pirenne his contribution (pp. 152-153), including his translation in French which Pirenne faithfully reproduces in her text. His contribution is an excellent example of the kind of argumentation written by an author blinded by a fixed idea typical of Beeston's papers.

On the compound expression bs, Pirenne notes that Robin "a relevé les tournures parallèles" in 3 texts, without indicating that they had already been identified as such in the edition. She also transcribes bnmw/strnw /b³ḫrr of Ja 561bis/8-9 as "BHN/TWSTRSW/B³HRR," thus, inventing a nonexistent ḫ, misplacing the separator and eliminating the diacritical mark of ḥ.

(a) Beeston mistakenly claims that "ᶜl/ᶜly would have rather be expected with the meaning of 'against'" (p. 152), with no reference at all. As a matter of fact, "against" is the 4th meaning of ᶜbr, but the 5th of ᶜl/ᶜly in SDDS (pp. 16 and 11, respectively).

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sb³ is ᶜd + rḥbtn/ - - /w³rḍ/ḥmyrm, and the most obvious parallel is

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The periope ᶜbr/mlk/sb³ is a determinative apposition to rḥbtn explaining the reason of the campaign of the Qat king to that city, as indicated by Beeston's translation of the periope (p. 153); cf. also above, bᶜbr of Ja 755/8, pp. 185, 186 and 196.

(d) Beeston then rejects that mlk governs both sb³/w³rḍ/ḥmyrm. That might have been Pirenne's initial understanding, but it is already demonstrated as erroneous in the preceding paragraph.

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It is quite true that there is no "specific mention of hostilities" for anyone who aprioristically take sb³ as having no military meaning, although this is its primary meaning.

(f) For his last 2 remarks, Beeston refers to W.W. Müller's paper in NESE, II, "p. 156," which deals with SharafT 31 (pp. 156-165). Only a voluntary blind person could make the preceding remark because it is absolutely unjustified. Indeed, (1) the expression sb³/ᶜd does not exist in the text; (2) in the expose of the 1st reason for thanks-

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(f) For his last 2 remarks, Beeston refers to W.W. Müller's paper in NESE, II, "p. 156," which deals with SharafT 31 (pp. 156-165). Only a voluntary blind person could make the preceding remark because it is absolutely unjustified. Indeed, (1) the expression sb²/ᶜd does not exist in the text; (2) in the expose of the 1st reason for thanks-

giving (ll. 3-7), sb²t (l.5) has a military meaning because of ġnm booty (cf. JaSIMB, p.445B), which precedes the noun in the same l.5 (cf. wġnmm/bkl/sb²t and booty during all the military campaigns). It remains possible, however, that in his deep knowledge of middle-aged society, Beeston knows cases - how many? - of knights and their squires returning from friendly, peaceful missions ... loaded with booty! (3) In the presentation of the 2nd reason for thanksgiving (ll. 7-14), the verb blt (ll.8 and 14), which has as primary meaning "to impose a duty" includes FIRSTLY the military expedition against (= bᶜbr) mlkn/bn/k(10)[²b]m/mlk²sr(ll. 9-10), and another son of that kᶜbm, ḫrtn, is already mentioned in Ja 660/11 and 15 as an enemy of the same šmr/yhrᶜš/ mlk/sb²/wdrydn/whḍrmwt/wymnt (e.g., ll. 6-7). Therefore, such an expedition against another son of kᶜbm should not surprise. Then, the author of the text specifies that sb²/ tty/sb²ty(11)[n] (ll. 10-11) he fought 2 battles during that expedition against mlkn. SECONDLY, the non-military meaning of blt (1.8) is expressed in SharafT 31/11 as mẓ²/ᶜdy (cf. mẓ² to arrive, e.g., in Ja 576/6). BOTH TYPES of duty are summarized in SharafT 31/12-14 as follows:

whmrh(13)w/²lmqh/²tw/bwfym/whfšḫn/b(14)[n/]kl/ḍblthw/mr²hw

and ²Ilumquh had vouchsafed to him to come back in safety and with delight [maṣdar of hfšḫ] fr[om] all duty [= both military and non-military] which had imposed upon him his lord.

The whole pericope of AM 757/8-10 = 208+177/7-9 may be translated as follows:

at the time in which he [= the Qat king] fought at Ruḥâbatân against the king of Saba², and (in) the land of Ḥimyarum.

For Ruḥâbatân, cf. JaSIMB, p. 475A and pl. F and G.

Note on Fa 102/3-4 (cf. ġnm of SharafT 31/5, above). — A.F.L. Beeston erroneously writes that "G. Ryckmans emended to bn/ġnmm/bn/hgrn; but the photo (kindly shown to me by Monsieur Robin) clearly has bn/ġnmhmw/bn/hgrn" (cf. Raydān, 4 [1981] 1982, p. 19).

This is a typical "Beestonian" argument, inventing and reading what he wishes to find. (1) Beeston's transcription of G. Ryckmans' text contains 2 errors. G. Ryckmans' transcription is " -/ġnmm(4)wbn/- " (cf. RyET, p. 62) and his translation of the expression as "son butin" (p. 63) proves that the 2nd m is a printing error instead of h; another printing error is the absence of the word divider between w and bn, as proven by Fakhry's facsimile in Archaeological Journey to Yemen (March-May, 1947), Cairo, I (1952), p. 116, (151): bnm/(4)(w)/bn. (2) Beeston did not at all need Robin's photo; one was already published by A. Fakhry, l.c., III (1951), pl.44B. (3) Beeston's reading (cf. "clearly") of "ġnmhmw" is wrong because the left half of h is damaged, and his 2nd "m" is inexistent in Fakhry's photo referred to above. Thus, ġnmh](4)w/, as it is already given in my copy of the text as MaN 25 made in the winter of 1951.

(7) The plural ²brt and its singular brt (ll. 14, 15 and 13, [15], respectively (pp. 148 and 182) are translated by Pirenne as "progéniture" and "pérennité," respectively. Pirenne's commentary (pp. 153-154) accumulates several errors.

(a) Her translation makes a distinction between singular and plural. This is an unheard-of novelty already contradicted by JaSIMB (p. 443B): "place, military campaign" and "to locate," this last point contrary to SDDS (p. 32) and BiDO (p. 60).

(b) She refers to Jacques Ryckmans' 1953 paper which she erroneously ascribes to "G. Ryckmans" (p. 153). This paper has already been rebutted in JaISAR, pp. 114-115, note 1. Note that Jacques Ryckmans' favored idea of "destination" which he values so much is not even mentioned in either SDDS (p. 32) or BiDO (pp. 60-61). He may take as some kind of consolation that RiLEQ (p. 48 = RiLIQ, p. 34) mentions it. Unfortunately, the latter adds that "there does not seem to me to be any cogent reason for regarding it as positively preferable."

(c) She claims that the meaning of place "ne convient [pas] au contexte de notre ligne 14" of AM 757 (p. 153).

No justification at all. For the very same remark by N.Nebes, cf. below, p. 232.

(d) Pirenne also errs when, in order to suit her fanciness, she looks in Datînah for the interpretation of a Sab word as well-known as brt found here in a very ordinary context. Indeed, she claims that her meaning of brt as "progéniture" "a l'avantage de correspondre à une racine sud-arabe" (p. 153), viz brt of LaGD, pp. 147-148. But, FrLaL (I, p. 103a) lists bart "terra plana et mollis." Note that G.R. Smith follows in J. Pirenne footstemps in the case of br (cf. above, p. 123).

(e) She also errs when she claims that her meaning of "progéniture" "s'accorde parfaitement au contexte de la ligne 14" (p. 153 on AM 757). The pericope of AM 757/12-14 = 208+177/11-13 is composed of 3 sections: lwfy (l.12) of the king, wwfysn (l.13) and wwfy/²brtsn (ll.13-14). Therefore, Pirenne's statement implies that the mention of the authors of the text represented by the pronoun -sn should be followed by that of their children. Such a thesis is contradicted by many texts. Here follow a few examples.

- Texts mentioning place names which include human beings; e.g.,
  the perfect parallel of NaNAG 19/3-4: lwfyhmw(4)w/wwfy/²bythmw;
  CIH 573/3: lwfyhmw/wwfy/bytn/slḥn (cf. also Fa 9/4 and 28/4);
  SharafT 21/2: lwfyhmw/wwfy/bytn/rymn (cf. also RES 3990/7-8).
- Texts mentioning animals:
  Ja 513/8-9: lwf[y](9)grybthmw/[w]wfy/²²blhmw;
  CIH 535/4-6: lwfy(5)hw/<w>wfy/bᶜrh(6)[w].
- Texts mentioning inanimate things:
  CIH 394/5: lw]fyhmw/wwfy/²tmr.

(f) Pirenne refers to Qat RÉS 3689/12: ʾsṯr/dn/brtn, which she claims to mean "la commémoration écrite de cette performance" (p. 154). This is a theatrical hocus-pocus. The text reads as follows: wtqdm/wsᶜšq/ʾsṯr/dn/brṯ(13)n/nbṯᶜm.

N. Rhodokanakis translates the direct complement as "den Ausfertigungen dieses inschriftlich kundgemachten (vorewigten) Aktes."

This whimsical, Beestonian-like translation includes a whole commentary of things foreign to the formula. Beside borrowing Rhodokanakis' "inschriftlich kundgemachten ... Aktes," Beeston completely disturbs the phrasaeology of the pericope when translating the pericope as follows: "The execution of this inscriptional record was supervised by Nbṯᶜm" (cf. BeLT, p. 11).

Beeston, who always seems to be hypnothized by syntaxic matters, gives in his translation of RÉS 3689 such a vivid example that nothing is respected provided his fancy is fully saturated. The above-listed pericope does not present any difficulty:

and Nabaṯᶜamm has directed and completed the lines of this place, viz

he has directed the entire engraving of the inscription located at this place.

Consequently, the pericope wwfysnn/wwfy(14)ʾbrtsn/rtdw/ᶜm/ddwnm/ǧlm(15)n/bnkl/msnkrm bn/brts (AM 757/13-15; cf. AM 208+177/12-15) may be translated as follows: and for their [= the women's] safety and the safety of (14) their [= the women's] places. They have entrusted to the care of ᶜAmm, Him of Dawânum, this statue (15) against any transporter from its place.

(g) Pirenne's translation of bnkl/msnkrm/bn/brts "as contre quiconque altérerait sa postérité" follows in the footsteps of Beeston's dissolute handling of the texts; msnkrm is an indefinite participle without a direct complement; cf. also ms[nkrm/bn/brts] of Ja 878/7. This reading is maintained against M.A. Bâfaqîh in CIASA, II, pp. 185-187, where the restorations suggested by the author are rejected, the paper being written by someone who can hardly claim to be an epigraphist.

[1] His reading of "drn" at the end of l.3 is erroneous; there is no n or even a section of it on either the photo or my squeeze. [2] He does not even attempt to read the letters of l.7; cf. JaSIMB, p. 346B. Bâfaqîh's elimination of this line which is not even mentioned in his commentary is inexcusable and shows at the very least a serious problem of vision. [3] The same problem is also attested at the end of l.4: his suggestion of "YḤL..." is incredible, to say the very least. [4] His restoration of "[ML?]" at the end of l.1 is erroneous. The bottom of the staff to the right of the separator after ǧmlm has a triangular appendix which makes it impossible for that staff to belong to m. [5] His use of 2 consecutive pairs of full brackets is unintelligible; e.g. "[SQ][NY]" (l.2), "Y[HQ][M]" (l. 4), "[BN][D]MRᶜLY" (l.5; the word divider is eliminated), and "[....R][TD]" (l. 6). [6] This last reading is erroneous: it should be [r]ṯd (cf. JaSIMB, l.c.). The upper circle of ṯ and most of the appendix of d also are still on the stone making it impossible to read any other letter. [7] On his imaginary restoration of "[ML?]" at the end of l.1, the author refers to "Bafaqih-Maqš Aṣbaḥī, dans Raydān, 2" (p. 186). Not only there is no mention of page(s), but even the title is wrong since the referred-to paper has 2 coauthors, Bâfaqîh and Robin (cf. p. 3 in Ar), and the names of the 2 coeditors are repeated on each right page of the paper as well as below the end of it. [8] Bâfaqîh also refers to "Hobin et Bron, ..., Semitica, XXIX, 1979, p." The page number is wanting again. But, it could not be given because the referred-to article has nothing to do with the suggested restoration.

(8) The 4th element of the final invocation of AM 208+177 and 757 is wnšbt/wᶜzyn/mndḫw/ḥrb (11.17-18 and 18-20, respectively).

a - Since the edition of the 1st text, AM 757, does not reproduce the photo of the left-hand side which contains the whole section (cf. already above, p. 214), it is impossible for the reader to know whether or not the separator after nšbt is authentic. However, as Pirenne restores without any reason a separator at the end of ll. 2 (cf. already above, p.214), 6 and 9, I consider it as Pirenne's invention, as she invented the 3 others.

b - Pirenne refers nšbt and ᶜzyn to an incomplete text published by A.F.L. Beeston in Oriens Antiquus, 1 (1962), pp. 47-49. But she does not refer mndḫw to the same text. Therefore, she was not yet aware (!?!?) that the complete text had been published eighteen years before the parution of her CIASA, viz I published the complete text, H2c, in G.W. Van Beek et al., Hajar bin Ḥumeid, Baltimore, 1969, pp. 338A-341A, photo on pl. 23b (p. 78, bottom); and the long commentary (including the rejection of Beeston's interpretation) on pp. 339B-341A on the pericope itself to which the reader is referred and which justifies the translation of it as "and [with] the persistent endurance of the two Mundiḥ of Ḥarib" (p. 338A). As was fully expected, Beeston rejected my translation in BeLT (pp. 19-20). It was only in JaMAR, XIV (1982), pp. 101-106 that I at long last published my rebuttal of Beeston's theory. Pirenne ignores - as could be expected - the last 3 publications in the same manner that she ignores all those that oppose her; cf., e.g., JaMAR, XIV, pp. 30-31: RÉS 3902+...+4155; XV (1987), pp. 38-39: Ja 2357, and pp. 42-43: Ja 2457. Pirenne still gives another testimony of her voluntary ignorance in her dealing with the deity of irrigation mnḥ (p. 155); cf. already JaSIMB, p. 169A.

Let us take 4 very simple questions on that deity m(n)ḏḫ.

[1] mnḏḫt was declared feminine by CIH (I, p. 67A, l.19 of the commentary on CIH 40/5), but the same CIH (III, p. 319A) states: "L.19, dele femininum" in the above reference

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N. Rhodokanakis translates the direct complement as "den Ausfertigungen dieses inschriftlich kundgemachten (vorewigten) Aktes."
This whimsical, Beestonian-like translation includes a whole commentary of things foreign to the formula. Beside borrowing Rhodokanakis' "inschriftlich kundgemachten ... Aktes," Beeston completely disturbs the phrasaeology of the pericope when translating the pericope as follows: "The execution of this inscriptional record was supervised by Nbṯ^{c}m" (cf. BeLT, p. 11).
Beeston, who always seems to be hypnothized by syntaxic matters, gives in his translation of RÉS 3689 such a vivid example that nothing is respected provided his fancy is fully saturated. The above-listed pericope does not present any difficulty:
and Nabaṯ^{c}amm has directed and completed the lines of this place, viz
he has directed the entire engraving of the inscription located at this place.
Consequently, the pericope wwfysnn/wwfy(14)ʾbrtsn/rtdw/^{c}m/ddwnm/ṣlm(15)n/bnkl/msnkrm bn/brts (AM 757/13-15; cf. AM 208+177/12-15) may be translated as follows: and for their [= the women's] safety and the safety of (14) their [= the women's] places. They have entrusted to the care of ^{c}Amm, Him of Dawânum, this statue (15) against any transporter from its place.

(g) Pirenne's translation of bnkl/msnkrm/bn/brts "as contre quiconque altérerait sa postérité" follows in the footsteps of Beeston's dissolute handling of the texts; msnkrm is an indefinite participle without a direct complement; cf. also ms[nkrm/bn/brts] of Ja 878/7. This reading is maintained against M.A. Bâfaqîh in CIASA, II, pp. 185-187, where the restorations suggested by the author are rejected, the paper being written by someone who can hardly claim to be an epigraphist.

[1] His reading of "ḏrn" at the end of l.3 is erroneous; there is no n or even a section of it on either the photo or my squeeze. [2] He does not even attempt to read the letters of l.7; cf. JaSIMB, p. 346B. Bâfaqîh's elimination of this line which is not even mentioned in his commentary is inexcusable and shows at the very least a serious problem of vision. [3] The same problem is also attested at the end of l.4: his suggestion of "YḤL..." is incredible, to say the very least. [4] His restoration of "[ML?]" at the end of l.1 is erroneous. The bottom of the staff to the right of the separator after ṣmlm has a triangular appendix which makes it impossible for that staff to belong to m. [5] His use of 2 consecutive pairs of full brackets is unintelligible; e.g. "[SQ][NY]" (l.2), "Y[HQ][M]" (l. 4), "[BN][D]MR^{c}LY" (l.5; the word divider is elimina-

ted), and "[....R][TD]" (l. 6). [6] This last reading is erroneous: it should be (c)l.ṯ.d (cf. JaSIMB, l.c.). The upper circle of ṯ and most of the appendix of d also are still on the stone making it impossible to read any other letter. [7] On his imaginary restoration of "[ML?]" at the end of l.1, the author refers to "Bafaqih-Naqš Miḥbaḥī, dans Raydân, 2" (p. 186). Not only there is no mention of page(s), but even the title is wrong since the referred-to paper has 2 coauthors, Bâfaqîh and Robin (cf. p. 5 in Ar), and the names of the 2 coeditors are repeated on each right page of the paper as well as below the end of it. [8] Bâfaqîh also refers to "Robin et Bron, ..., Semitica, XXIX, 1979, p." The page number is wanting again. But, it could not be given because the referred-to article has nothing to do with the suggested restoration.

(8) The 4th element of the final invocation of AM 208+177 and 757 is wnšbt/w^{c}zyn/mndḫw/ḥrb (ll.17-18 and 18-20, respectively).

a - Since the edition of the 1st text, AM 757, does not reproduce the photo of the left-hand side which contains the whole section (cf. already above, p. 214), it is impossible for the reader to know whether or not the separator after nšbt is authentic. However, as Pirenne restores without any reason a separator at the end of ll. 2 (cf. already above, p.214), 6 and 9, I consider it as Pirenne's invention, as she invented the 3 others.

b - Pirenne refers nšbt and ^{c}zyn to an incomplete text published by A.F.L. Beeston in Oriens Antiquus, 1 (1962), pp. 47-49. But she does not refer mndḫw to the same text. Therefore, she was not yet aware (!?!?) that the complete text had been published eighteen years before the parution of her CIASA, viz I published the complete text, H2c, in G.W. Van Beek et al., Hajar bin Ḥumeid, Baltimore, 1969, pp. 338A-341A, photo on pl. 23b (p. 78, bottom); and the long commentary (including the rejection of Beeston's interpretation) on pp. 339B-341A on the pericope itself to which the reader is referred and which justifies the translation of it as "and [with] the persistent endurance of the two Mundiḥ of Ḥarib" (p. 338A). As was fully expected, Beeston rejected my translation in BeLT (pp. 19-20). It was only in JaMAR, XIV (1982), pp. 101-106 that I at long last published my rebuttal of Beeston's theory. Pirenne ignores - as could be expected - the last 3 publications in the same manner that she ignores all those that oppose her; cf., e.g., JaMAR, XIV, pp. 30-31: RÉS 3902+...+4155; XV (1987), pp. 38-39: Ja 2357, and pp. 42-43: Ja 2457. Pirenne still gives another testimony of her voluntary ignorance in her dealing with the deity of irrigation mnḥ (p. 155); cf. already JaSIMB, p. 169A.

Let us take 4 very simple questions on that deity m(n)ḏḥ.

[1] mndḥt was declared feminine by CIH (I, p. 67A, l.19 of the commentary on CIH 40/5), but the same CIH (III, p. 319A) states: "L.19, dele femininum" in the above reference

because in the meantime CIH (I, p. 311B, commentary on CIH 294) had affirmed that mndḫt was a plural. [2] Besides, Pirenne also states that "il ne s'agit pas non plus de divinités particulières, mais bien d'un nom commun." This is erroneous, cf. already JaP, pp. 125-127. Among the many examples contradicting her theory, cf. Ga 35/3-6:-/wrtdw/ʿttr/wwdm(4)wkl/ʾlʾlthmw/wmndḫmw/wšmš(5)hmw. [3] Pirenne refers to M. Höfner disputing that mndḫt is the feminine of mndḫ (cf. fn 3). Thirteen years before Höfner's paper, JaP (p.125) already speaks of "pluriel MUNDIḪAT (mndḫt)." [4] Pirenne endorses A. van den Branden's opinion on mndḫ being "protecteur." For A. van den Branden's opinion and C. Robin's endorsement of it, cf. JaMAR, XIV (1982), pp.271-274. [5] Pirenne publishes Beeston's statement according to which

"We must see a difference of function between MNDḪ and the ŠYM."

What Beeston is referring to is the divine name ʾnby/šymn (11.16-17; also 17-18) ʾAnbay, the Patron," which immediately precedes the above-listed pericope. But, nobody has ever claimed that their functions were or should be identical or similar simply because that is contradicted by the whole SA material. What is, thus , the purpose of Beeston's statement unless its author wishes to ... break into an open door by writing a truism, or to write one of his innumerable "trite remarks" (cf. above, p. 197), or to shunt the whole question into a meaningless siding?

D - Ḥaḍr (cf. Mas-Gellas, l.c., pp. 183-184).

Robin deals with the Ḥaḍr verbal forms.

1 - "qtlw, 3rd person masculine dual" (p. 183) is the 1st section with reference to Ja 923, 948, 969 and CaTh 4.

a - It is incomprehensible that, in order to prove his thesis that the verbal form qtlw is a real dual in several texts having 2 subjects only, he fails to refer to Ja 402 which reads as follows:

šfsy/wʾms	1	Šafsay and his mother
nḍrt/sqnyw/m	2	Naḍrat have dedicated to
rʾsmn/syn/dʾ	3	the lord of them both, Sîn, Him of ʾI-
lm/bsmhrm/	4	lum in Sumhurâm,
hwfy/grbsm	5	for the protection of their body
wmlksm	6	and of their king.

(cf. AIAAE, p. 42, and photo in fig. 61 in pl. 33, bottom).The text is clear: there are only 2 persons as authors of the text: a man and his mother; no other human being who could in one way or another be added to the 2 coauthors is mentioned directly or indirectly. Yet, if the dual pronoun -smn concords perfectly with the 2 coauthors,the twice-used plural pronoun -sm is no less evident, and regardless of their respective pronunciation, they surely remain basically different. This fact proves the use of the

plural instead of the dual. Therefore, sqnyw remains a plural form which may be used as a dual in the same manner that the plural pronoun -sm is used instead of the dual -smn without becoming a dual itself.

b - Robin's analysis of CaTh 4 is centered on the dual pronoun -smn (1.6) which, in his judgement, clenches his interpretation of the verbal form sqnyw (1.3). The preceding inscription already proves that such is not the case.

[1] Preliminary remarks not made by Robin. - Robin limits himself to saying that "le début est partiellement mutilé." The setup of the letters of 11.1-2 contradicts the restoration of "rmy" at the end of 1.1 by G. Ryckmans (cf. G. Caton Thompson, The Tombs and Moon Temple of Hureidha (Hadhramaut), Oxford, 1944, p. 158, and photo in pl. 63, no.3). There is no room for a single letter after kb[r/r]. To the right of ʿqnm (1.2) the photo shows the lower ends of 2 vertical strokes. Therefore, I suggest reading 11.1-2 as follows: -/ḫbsm/kb[r/r](2)[my/w]rḍ]ʿqnm/ʾmnw/-

- Ḫabsum, the lead[er of Ha](2)may and](Dû)ʿaqnum, the commissioners of (cf. Ar ʾamîn). The dqtlm nominal derivation is attested in Ḥaḍr dkrm of Ja 972/2 (cf. also Sab dqʿltm of Ja 3256/3 in JaMAR, XVIII [1995], p. 81F); the relationship between ʿsmm and (d)ʿqnm is not specified in the text. G. Ryckmans was correct in suggesting the mention of a 2nd person in 1.2, but his alignment of ʿ of (d)ʿqnm immediately below the separator after bn of 1.1 is wrong because that separator is in the axis of the space between q and n of the same name.

[2] The case of CaTh 4 is parallel with that of Ja 402 with the plurals ʿmnw (1.2) and sqnyw (1.3) and the dual pronoun -smn (1.6). In the present text, the emphasis is on ʾmnt (1.6): the 1st author declares that sḫḍs/w(6)gsm/bʾmntsmn/bʾrhn he made new and massively built the well with the safe materials [Ar ʾamân] of [= provided by] them both, viz nobody but them both is responsible for the materials used in that building. This is another case of the main or 1st author attributing to himself the whole work. Note that the editor translates the singular sḫḍs as a plural: "ils ont renouvelé;"and that the photo of pl.LXIII, no.3 shows the letter w engraved on top of non-obliterated b, and this suggests that the engraver had forgotten wgsm and had started bʾmnt (1.6).Note also the article -hn of bʾrhn (1.6) as above, p. 191: ʿtwfhn.

c - The principal feature of Robin's thesis is 3 texts from al-ʿUqlah, viz

Ja 923: 2 subjects šwʿw/mrʾsmyn (1.2),and slḥsmyn (11.4-5);
Ja 948: 2 subjects šwʿw/mrʾsmyn (1.2), and
Ja 969: 2 subjects šwʿw/mrʾsmyn (1.2).

Here again, the parallelism with Ja 402 is visible. But, what is incomprehensible is that Robin would make such a fuss about the plural form šwʿw which is used with either

the dual -smyn = -smn (in defective writing) and the plural -sm in order to emphatically boast "to come across new forms that up until now were inadequately analyzed" (p. 185). It is irrational to take such a stand because of his remarks on Hôr Rôrî (p. 184; cf. below), he himself refers to "ci-dessus p.12" (the page of his manuscript), which is p. 174 of his paper where he analyzes Ja 641, which is dealt with above, pp.184-185. — C. Robin claims that tqblm of Ja 923/6 must be corrected to "tnblm" (cf. JA, 279 [1991], p.19, fn 1). This is a gross error (cf. JaUT, p.40 and photo in pl. IB; and JaSIMB, p.384 and fn 25). Even J. Pirenne, who also outdid herself to find faults with JaUT, accepted tqblm (cf. Témoins écrits de la région de Shabwa et l'histoire, Paris, 1990, p.107 and pl. 70; I will publish a detailed review of this book at a later date); so did before her M.A. Bâfaqîh in his Âtâr an-nuqûš al-ᶜUqlah, Cairo, 1967, p. 62. However, Bâfaqîh changed his mind in Muḫtârât min an-nuqûš al-yamanîyat al-qadîmat, Tunis (1985), where he transliterates the letter q as "ṇ" (p. 327), viz doubtful, and that letter becomes certain in p. 403 without any justification! Bâfaqîh's commentary (p. 328) claims that the reason of his reading is "kadâ fî-l-taṣwîr (wa-laisa tqblm kamâ fî nushat Jamme)." If his 2nd statement is correct because it simply repeats JaUT's p.40, his 1st affirmation is untrue: JaUT's photo in pl. IB clearly shows q, certainly not "ṇ" or "n." Furthermore, would - as he claims - the photo clearly show "n," why does he consider that letter as doubtful, "ṇ," in his copy of the inscription (p.327)? That is quite illogical. Moreover, Bâfaqîh's statement proves that, while working at al-ᶜUqlah, he did not decipher the texts from the stone, as he bases his note on "taṣwîr" = photo. JaMAR, XV (1987), p. 78, already stated that "Bâfaqîh's transliteration of the texts in Ar letters was not made from his own copies of the originals, but was a mere transliteration of JaUT's transcriptions in SA letters." My copy from the stone has tqblm. Finally, the reason that prompted Bâfaqîh to misread the text remains unknown. However, the simple fact that Robin does not refer to Bâfaqîh's book, of which he himself is a coauthor, may suggest that he, Robin, in fact is the inventor of the error. In this case, the reason why Robin voluntarily misread a text, also remains unknown.

2 - The 2nd item of Robin's presentation of Ḥaḍr verbal forms is qtln, 3rd personal feminine plural (cf. Mas-Gellas, l.c., p. 183) in Ja 919. As duly noted by Robin, the form was correctly analyzed in the edition (cf. JaUT, p.39). It is also interesting to point out that MüNAI (p.62) endorses JaUT's translation of qršhtn the Qurairšitinnen." On the contrary, it is utterly laughable to see Robin translating the expression as "gardiennes de bétail"!?!? ... as if he suggested himself - quite indirectly, of course, - to be called "Enrobé" au lieu de "Robin".

3 - Robin's last item in his study of the Ḥaḍr verbal forms is that qtl is sometimes attested "là où on attendrait fᶜlw, 3e pers. masc. duel ou plur." (p. 184): this "point d'orgue" fittingly closes the kaleidoscopic saga of his dogged mirage.

a - As the example of the singular verb instead of the dual, Robin refers to RÉS 4182, which he transcribes as "Šyᶜn bn ʾbydᶜ [w]-bn-[s] sqny Syn."

Even at 1st sight, how stupid of Robin to here again apply his crazy transliteration system and, in so doing, to impose his own demential interpretation upon his reader! Also, even at 1st sight, how irrational it is to base a serious conclusion exclusively on A SINGLE WORD composed of 4 letters, 2 of which are restored and a 3rd one already proven to be wrong more than 30 years ago! So, Robin did select a sham example! In 1962, under his no. 620, G. Ryckmans had corrected the whole reading of "[w]bn[s" to "ᶜḍn ᶜAḍân (?)" (cf. Le Muséon, 75 [1962], p. 217). Note that G. Ryckmans refers to the "photo de Kh. Y. Nami" in Ṣalâḥ al-Bakrî al-Ŷafaᶜî, Taʾrih ḥaḍramawt as-siyâsî, Cairo, 1936, "t. I, p.70." This reference is erroneous: what al-Bakrî reproduces on p. 57 is a reduction of the plate of p. 176 of D. van der Meulen and H. von Wissmann, Ḥaḍramawt, Leyden, 1964 (reprint), and it is a crude retracing of the preceding facsimile that G. Ryckmans reproduces on p. 215 of his paper. Then, WiZAG (1968) publishes not only a good facsimile of the antiquity made in 1931 (p. 44, bottom) but also repeats "ᶜḍn[.] ᶜAḍnā[n?] (p.45, fn 100), WiHöBGS's (1953) translation of that central section as "ᶜḍn. ᶜAḍn[ân?]" (p.135, fn1). A.F.L. Beeston had erroneously pointed out that WiHöBGS does "propose a new reading of the text (with ᶜḍn following the name of the dedicant)" (cf. AfO, 117 [1954-1956], p. 165B) without noticing that H.Y. Nâmî had already given it in his 1943 Našr nuqûš samîyat qadîmat min janûb bilâd al-ᶜarab wašarḥuhâ, Cairo, p. 107. Furthermore, Beeston miscopied WiHöBGS by transcribing "ᶜḍn[.]" as "ᶜḍn". Finally, the photo published by H.Y. Nâmî (cf. l.c.) shows that there is room for two letters between ᶜḍn and /sqny; this is also evidenced by H. von Wissmann's 1968 facsimile. Thus, I suggest reading ᶜḍn[yn] [the] ᶜAḍan[ite], and the 1st half of the text may be read as follows: šyᶜn/bn/ʾbydᶜ/ᶜḍn[yn]/sqny/syn/-

Šayᶜân, son of ʾAbyadaᶜ, [the] ᶜAḍan[ite,] has dedicated to Sîn ...

Therefore, Robin's mirage vanishes in the thin air because it was only smoke: there is only one subject of the verb sqny, and there is no question of "et son fils," as he puts it, and, beside, his translation does away with the 2 pairs of brackets evident in his transliteration.

b - As the example of a singular verb used instead of a plural, Robin takes Hôr Rôrî 4 (p.184). He still refers to J. Pirenne's publication of an incomplete text (cf. The Journal of Oman Studies, 1 [1975], pp. 86 and 89), thus haughtily ignoring the republication of the beginning of the inscription in JaMAR, IX (1979), pp. 84-85 and 89, with

the corrections of Pirenne's reading of ll.1-3 (including the reading of almost the 1st 2/3 of l.1, which were not deciphered by her) and ll.4-7. For the whole text, cf. AlAAE, p. 43, and photo of a latex squeeze in fig. 65 of pl. 35. Of my preceding studies based on 2 latex squeezes and photos made before the stone was damaged, Robin apparently does not care: he prefers to reproduce Pirenne's errors. This is another example (cf. already above, p. 210) of Robin scorning some valuable documentation gathered before his time. — The singular tbᶜ which immediately follows the plural relative pronoun ʾsd may be explained by the fact that the 5 men, who are the antecedent of ʾsd, were seen as a mere unit following and accompanying the commander of the troops of Ḥaḍramawt; and the singular verbal form is logical. Those 5 men most probably were already the group of servants of the military commander before they left for Sumhurâm.

Robin finds himself unable to determine which one of the 2 following suggestions is correct. [1] The singular verb tbᶜ may be in concord only with "l'un des sujets;"this is Robin's 1st suggestion.This hypothesis is wholly inexplicable (cf. immediately below) because the 5 authors come from different families and are not given any title; therefore, nothing in the text even suggests that there was a chief among them.

[2] Robin's 2nd suggestion is that the singular tbᶜ may be a defective writing. This is as easy as gratuitous;it is, however, an unproven suggestion because its only apparent justification is a superficial analysis of the inscription. The authors are considered as a unit and all the more so because they never refer to themselves individually in the text where not a single personal pronoun is to be found. As soon as their common mission is described as tbᶜ/wšyᶜ,the text concentrates only on their master, ʾbytᶜ, and completely forgets about those 5 authors. Furthermore, the preceding understanding of those 5 men as one unit or group makes improbable the hypothesis, of which Robin did not think, of a haplography with the w introducing šyᶜ, viz tbᶜ<w>/wšyᶜ. But such a hypothesis cannot even be taken into consideration unless no other solution is possible, and this is most certainly not the case.

It is in connection with his study of RÉS 4182 and Ḥôr Rôrî 4 that Robin refers to his previous analysis of Gl 1720, CIH 496, Ja 2354 and Ja 641, which were examined above, cf. respectively, pp. 178 and 181-185.

E - Summary.

There can hardly be any doubt that Robin's study of the SA verbal forms is quite definitively negative: a continuous cerebral "fantôme" (cf. Robin,p. 176) inspired by a lustily unquenchable pruritus of novelty. When the reader ponders that Robin delivered his dreamy and deluded concoctions during lectures in the frame of "une initiation à l'épigraphie sudarabique" (cf. already above, p. 177), he can only regret

extremely, feeling an immense sorrow for Robin's auditors who were so wholly outcast as to be at the wrong place at the wrong time, and to listen to the proclamations spewed by a fanatic champion of partisanship. With the exception of the cases analyzed accurately before, his interpretations of the verb forms mentioned above are so far off because his systematization of those forms is a voluntarily protracted chimera caused by personal ambition at the expense of the objective analysis of the texts.

F - List of abbreviations.

ABADY: J. Schmidt et al., Archäologische Berichte aus dem Yemen, Mainz, I-VI (1982-1993). - AlAAE: F.P. Albright, The American Arachaeological Expedition in Dhofar, Oman, 1952-1953, Washington, 1982. - AvGIAS: A. Avanzini, Glossaire des inscriptions de l'Arabie du Sud 1950-1953, Florence, 2 vol., 1977 and 1980.

BaRoNAH: M.A. Bâfaqîh and C. Robin, نقش اصبحي من حصي, in Raydān, 2 (1979), pp. 11-23. - BeDGSA: A.F.L. Beeston, A Descriptive Grammar of Epigraphic South Arabian,London, 1962. - BeLT: A.F.L. Beeston, The Labakh Texts (with Addenda to 'The Mercantile Code of Qataban'), London, 1971. - BeSG: A.F.L. Beeston, Sabaic Grammar, Manchester, 1984. - BiDO: J.C. Biella, Dictionary of the Old South Arabic Sabaean Dialect, Chico, 1982.

CIASA: Corpus des inscriptions et antiquités sud-arabes, Louvain, 2 vol., 1977 and 1986. - CIH: Corpus inscriptionum semiticarum. Pars Quarta: Inscriptiones himyariticas et sabaeas continens, Paris, 3 vol, 1889, 1911 and 1930; Plates, 3 vol., 1889-1932.

FrLAL: G.W. Freytag, Lexicon arabico-latinum, Halles, 4 vol., 1830-1837.

Gl: E. Glaser.

Ḥaḍr: Ḥaḍrami. - HöASG: M. Höfner, Altsüdarabische Grammatik, Leipzig, 1943. - HöISH, II: M. Höfner, Inschriften aus Ṣirwaḥ, Ḥawlān (II. Teil), Vienna, 1976.

Ja: A. Jamme. - JaCIASA: A. Jamme, Corpus des inscriptions et antiquités sud-arabes, in Orientalia, 48 (1979), pp. 260-284. - JaCMYE: A. Jamme, Carnegie Museum 1974-1975 Yemen Expedition, Pittsburgh, 1976. - JaISAR: A. Jamme, Inscriptions sud-arabes de la Collection Ettore Rossi (En appendice, la Collection Tazzer), in RSO, 30 (1955), pp. 103-130 et pl. 1-3 in pp. 128-130. - JaMAR: A. Jamme, Miscellanées d'ancient arabe, Washington, II (1971), VI (1974), IX (1979), XII (1982), XIV (1982), XV (1987), XVI (1988), XVII (1989), and XVIII (1995). - JaMIL: A. Jamme, Minaean Inscriptions Published as Lihyanite, Washington, 1968. - JaP: A. Jamme, Le panthéon sud-arabe préislamique d'après les sources épigraphiques, dans Le Muséon, 60 (1947), pp. 57-147. - JaSIMB: A. Jamme, Sabaean Inscriptions from Maḥram Bilqîs (Mârib), Baltimore, 1962. - JaUT: A. The al-ʿUqlah Texts, Washington, 1963.

LaAEL: E.W. Lane, An Arabic-English Lexicon, New York, 8 vol., 1863-1893. - LaGD: C.

Comte de Landberg, Glossaire dâ'inois, Leiden, 3 vol., 1920 - 1942. - LaH: C. Comte de Landberg, Ḥaḍramoût, Leiden, 1901.

Min: Minaean. - MüNAI: W.W. Müller, Neuinterpretation altsüdarabischer Inschriften, RES 4698, CIH 45+44, Fa 74, in AION, 36 (1976), pp. 55-67 and pl. 1-4.

NA = North-Arabian. - NESE: Neue Ephemeris für Semitische Epigraphik, Wiesbaden, 3 vol., 1972 - 1978.

Qat: Qatabanian.

RÉS: Répertoire d'épigraphie sémitique, Paris, 8 vol., 1901-1968. - RiLEQ: S.D. Ricks, A Lexicon of Epigraphic Qatabanian, Ann Arbor, 1984. - RiLIQ: S.D. Ricks, Lexicon of Inscriptional Qatabanian, Rome, 1989. - RoHT: C. Robin, Les hautes-terres du Nord-Yémen avant l'Islam, Istanbul, 2 vol., 1982. - RoPH: C. Robin, Le pays de Hamdān et Ḥawlān Quḍā'a (Nord-Yemen) avant l'Islam, Paris, 1977. - RoRycBG: C. Robin et J. Ryckmans, Dédicaces de bassins rupestres antiques à proximité de Bâb al-Falaǧ (Mârib), in ARADY, I (1982), pp. 107-115 and pl. 38-41. - RoRycIA: C. Robin et J. Ryckmans, Les inscriptions de al-Asâḥil, ad-Durayb et Ḥirbat Sa'ûd ..., in Raydān, 3 (1980), pp. 113-181 and 30 plates outside the text. - RyET: G. Ryckmans, Epigraphical Texts, Cairo, 1952. - RycRSA J. Ryckmans, Religion of South Arabia," in The Anchor Bible Dictionary, VI (1992), pp. 171B-176A: a jumble of short notes relating to tunes, old and new, of the members of the laudemus invicem gang without any textual references which they haughtily claim to summarize. Cf. also above, p. 30, for M.C.A. Macdonald's contribution to the same.

SA: South-Arabian. - Sab: Sabaean. - Saf: Safaitic. - SDDS: A.F.L. Beeston - M.A. Ghul - W.W. Müller - J. Ryckmans, Sabaic Dictionary - Dictionnaire sabéen - al-mujam as-saba'i Louvain-la-Neuve, 1982; cf. my review in JaMAR, XIV (1985), pp. 62-269.

Th: Thamudic.

WiZAG: H. von Wissmann, Zur Archäologie und antiken Geographie von Südarabien, Istanbul, 1968. - WiHöBGS: H. von Wissmann und M. Höfner, Beiträge zur historischen Geographie der vorislamischen Südarabien, Wiesbaden, 1953.

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August 2, 1993;
revised in late 1995.

Addendum to p. 214, end of l.12.

It is utterly incredible that Pirenne would write the siglum as "177+208" - and Robin fails to point out the error - although AM 208 is the supper section. It is also incredible that Pirenne would claim that "le bloc est brisé en trois morceaux" (p. 179). It should be pointed out that that there is no error for Pirenne when she writes "177+208". It is indeed her habit - denounced once more above, p. 216 - to use for the very same antiquity the very same siglum, read once from left to right and then from right to left; e.g. "98.RN : p 7 : C 21" (cf. I, p. 239) and
"C 21 : p 7 : 98.Rn" (cf. l.c., pp. 239 and 273).

Therefore, "177+208" is exactly the same as "208+177" for Pirenne, viz forward and backward, it is all the very same difference!!!

AM 208 is broken in 4 fragments; the missing fragment removed the last 2 letters of
front: ll. 1-8 + the upper 3/4 of l.9; and l.18: bt;
left-hand side: ll. 15-20.
AM 177: front: lower 1/4 of some letters of l.9 + ll. 10-14.

VIII

Brief review of
N. Nebes, Die Konstruktionen mit /fa-/ im Altsüdarabischen: syntaktische und epigraphische Untersuchungen
(Akademie der Wissenschaften und Literatur - Mainz.
Veröffentlichen der Orientalischen Kommission, Bd. XL),
Wiesbaden, 1995.

I wish to take advantage of the supplementary note on AM 208+177 (cf. above, p.230) to add a short review of N. Nebes' booklet. After half a day sounding here and there, I had collected enough material for about a 75-page review. It would be a waste of time to write them all because it always boils down to the very same. It is fortunate that Nebes could not wait any longer: as soon as he could, he made very sure (p.12) that the reader knew his basic secret, viz that the 2 unconstested beacons that guided and inspired his whole study and research were A.F.L. Beeston and especially ("besonders") W.W. Müller. The reader is grateful to Nebes for this public acknowledgment informing him of what had appeared obvious even at the 1st sight of the booklet and during a few soundings at random. The reader knew from the start that he had to face another onslaught of pan-classical Arabism which shows its ugly face already in the title itself of the booklet that speaks of "/FA-/" (on cover and pp. 1 and 3) = "/fa-/" (p. 4). What a beautiful example of vain show-off which is consistently repudiated afterward by the simple mention of "f-" in all its following pages!

The text AM 208+177 is listed by Nebes, but as "AM 177+208" (pp. 251 and 298, where "252" must be suppressed). - In Nebes' copy of ll. 9-13 (p. 251), "[n]" of l.10 must be corrected to (n); and "bṯ" of ll.11-12 must also be corrected to (b)[ṯ]. The pericope of ll.9-13 is the same as that of AM 757/10-14 (except for the royal name). Nebes' translation of those ll.9-13 reminds the reader of N. Rhodokanakis' turgid style, crammed with A.F.L. Beeston' extravaganzas, including those of the Sabaic Dictionary... (1982) by the same Beeston and his laudemus invicem club among whom is - everybody knows - W.W. Müller. A few examples follow.

The verb wʿd to continue is reduced to the state of a temporal adverb, viz "again, furthermore, in addition, in future" (cf. Sabaic Dictionary..., p.167). It surely takes a distorted mind to consider any of those 4 adverbs as identical or even similar and, worst of all, to see in any of them the meaning of "to continue." Nebes slavishly retains the 2nd and the 4th meanings as "weiterhin" in AM 208+177/11, and "in Zukunft" in AM 757/10-11. How blinding and enslaving can such a clientage really be!

For ṣdq to act justly becomes "(das, was recht und billig) gewähren" in both places!

For mngw security measure , it becomes "Ding." What could be vaguer than "Ding"?
For wfy safety: Nebes chose Sabaic Dictionary's last, incorrect meaning "well-being" (p. 158); as if "safety" and "well-being" were synonymous (cf. already above, p.215), and he translates it as "Wohle."
For brt place. Nebes invents a brand-new, nonsensical meaning for the word, viz "Handlung" - of course, he had already used "Ding" for mngw (cf. above) -, unless it would be the writer's understanding of the word "occasion" also given by the Sabaic Dictionary... as its 3rd and last meaning. Finally, to justify his parody, Nebes' fn 17 (p.251) does not shrink from stating that the sense of place "passt in diesem Kontext nicht." Same remark as J. Pirenne's, cf. above, p 219,(c). It shows that Nebes was trained and coached by the wrong persons, but it is precious in helping the reader to understand much better the basis of Nebes' other nonsensical or irrational suggestions.
P. 249, fn 3. - Nebes claims that "/f-ykbr" is found in RES 3688/6 by S.D. Ricks, Lexicon of inscriptional Qatabanian, Rome, 1989, p.127. True, Ricks lists "Q 66/6" (= RES 3688/6), but the reference is an error to be suppressed. And Nebes copies "/f-ykbr" as if from Ricks. But, Ricks did NOT write it anywhere ..., not even in p.83, under kbr. Finally, Nebes claims that his invention "/f-ykbr" must be corrected to "byg^{6}fy/kbr" according to his idol A.F.L. Beeston, The Labakh Texts, London, 1971, p.12, where the pericope is read as follows: " -/kl/gfym/byg(6)fy/kbr" and the commentary formally states that "gfy is clear on the photograph" (p. 14).
I have repeatedly emphasized in the preceding pages that Beeston was one of those so-called theorizers who regularly confuse g and l (e.g., pp. 4, 202-204, 210, 211 and 223). Here is another example of his error: I deny categorically Beeston's truthfulness of his preceding testimony on the basis of my copy,
my photo,
my black-and-white diapositive,
my color dispositive, and
my squeeze of the o r i g i n a l : viz
Beeston "gfy" { is t w i c e an e r r o r instead of lfy, and
is fortunately omitted by Ricks' Lexicon ... (pp. 35-40), an important feature that Nebes' honesty prevented him from informing the reader. What is even worst — and Nebes remains silent as the grave! — the correct reading of the pericope is given by the same Ricks' Lexicon..., p.93 under lfy, which reproduces my Miscellanées ..., III (1971), p. 60, where "w" of "wlfy" obviously is a typing error for b. The pericope reads as follows: -/wkl/lfym/by(6)lfy/kbr/-
"and all profit [which] shall collect the kabir of" (cf. my Miscellanées..., l.c.).

Ja 2360/12-13 (pp. 249-250). - Nebes reads the short pericope as follows:
"wdtn/ʾrḍtn/[wʾs]^{13}ṯrn/- und . diese Areale und Landstücke."
I wish first of all to denounce Nebes' dishonesty when he speaks of "Ergänzungen nach Transkription von A. JAMME" (cf. fn 9 of p. 250). It is the text (WITHOUT Nebes' parentheses) which I deciphered from the original, an essential information that Nebes is not honest enough to give the reader. Nebes' dishonesty is also illustrated, in order to avoid destroying the testimony of his "Pi 6/12 liest bs" (cf. l.c.), by the fact that he hides from the reader (once more!) that Pirenne's text must be corrected in 17 (seventeen) places; this apodictically proves that Pirenne did NOT decipher the text from the rock — cf. above, p. 47, and it is well-known that it was not her habit of doing in the field what for her was to be a desk work — but exclusively from her photos. This fact is confirmed by the only museographic detail given by her on the text, viz "les lettres ont de 3 à 4 cm de hauteur" (cf. Hommages à André Dupont-Sommer, Paris, 1971, p. 120). This information is manifestly wrong: the letter height measures from 6, 5,5, 5, 4,5 to 4 cm (cf. my Miscellanées..., III [1972], p. 22). Moreover, by saying "de 3 à 4 cm de hauteur," Pirenne proves that she only guessed at the height of the lower lines which she did not measure ... because any human capable of seeing can read the difference between 3 and 4 cm on any kind of meter. Finally, even a myopic would at 1st sight see the glaring oversize of 1.1 compared with the others. It is most unfortunate that Nebes would end his scandalous feat by justifying his set of parentheses of his erroneous "[wʾs] of 1.12 by saying: "Auf dem gegenüber von Pi 6 abgebildeten Footo der Inschrift sind die fraglichen Zeichen nicht identifizierbar" (cf. fn 9 of p. 250).
Similar reasoning unfortunately is a loathsome characteristic of present-day research, as shown above by J.F. Healey - G.R. Smith in the case of Nabataean JsaN 17. The 3 letters involved are indeed "fraglich" = doubtful o n l y in Nebes' distorted mind, and his dishonesty is perfectly manifest. My tracing from my color diapositive as well as my copy from the original, a double first-hand feat which Nebes will never be able to brag about, but which his haughtiness cannot accept and forces him to stoop so low as to hide it from the reader, clearly show those 3 letters.
Finally, Nebes' translation contain an evident tautology. Indeed, he should have added an extra fn to explain the difference between "Areale" and "Landstücke" in the pericope, viz between "ground" and "plot." And Nebes backs up his finding with a reference to "Bee St 2.23" (cf. l.c., fn 10) = Raydān, 3 (1980), p.23, where A.F.L. Beeston categorically states that: "I do not see how one can possibly here interpret ʾs^{1}ṯr as 'writings'. It surely must means 'tracts of land'."
Because of an unknown reason (of course and as usual), this most evident certainty va-

nishes in the thin air and becomes "?" = "dubious" in Sabaic Dictionary..., pp. XVIII and 129. Obviously enough, no reason has ever been given for such a downgrading, and Nebes is not honest enough to inform the reader of A.F.L. Beeston's about-face. It was beyond the Oxonian "Luminary"'s comprehension that an inscription could also serve as a cadastral landmark. Finally, how ridiculous of Nebes to force the reader to consult another booklet for "Bee St 2" when there is so much wasted blank in his fn 10!

The name ʿdbh of Ja 576/11. - This 4th and last example is an exceptionally unmatchable specimen of Nebes' dishonest method aiming at flattering his idols at the expense of the duly published documentation. Yet, this example was not foreseen to be the last of this review; but, it is unquestionably well suited "pour terminer en beauté." Nebes reproduces a pericope of Ja 576/11 (p. 22) mentioning the name of ʿdbh/mlk/ʾksmn ʿAdbah, king of ʾAksûmân, with the reference to fn 4 which reads as follows:

"[4]) Oder WḎBH zu lesen, vgl. Mü Abess 162: „Der sich dahinter verbergende äthiopische Name ist entweder ʿzbh (...) oder aber, falls man am Wortanfang statt des ʿ ein *w* liest, also *wḏbh* (...), *wzbh*; dies würde dem Ουαζηβα bzw. WZB = Wazēbā der aksumitischen Münzen entsprechen (...)". "

This is a typical example of obsequiously servile toadyism undoubtedly approved by W.W. Müller. Nebes' mention of the siglum "Mü Abess" is another of his irrational moves forcing the reader to look somewhere else for the real name (cf. above, p. 233, the similar case of "Bee St 2;" and there are many other cases). The 1st result of Nebes' trick is to prevent the reader from knowing that the paper of W.W. Müller, his most trusted beacon, was printed in Neue Ephemeris für Semitische Epigraphik, III (1978), where the author's text refers after "wḏbh" to J. Ryckmans in Le Muséon, 70 (1957), p. 84, who takes it for c e r t a i n without even mentioning my reading of ʿdbh from the stone itself, giving another good example of his doggedly partisan obscurantism.

Of course, here again, Nebes is not honest enough to even refer to the statements of my Miscellanées..., X (1979), p.29 (cf. also above, p.16), viz

"Another important feature of Ja 576 is the mention of an Ethiopian king called ʿdbh (l. 11) "ʿAḏbah."

G. Ryckmans had read the name as "wḏbh" (9) "Wazebah" (10). In 1957, I stated that "the name of the Ethiopian king is ʿdbh and not wḏbh; its reading is beyond doubt on the stone" (4). Yet, almost twenty years later, in 1976, A.F.L. Beeston still maintains without any justification of any kind (except, of course, his allegiance to the "laudemus invicem" club) G. Ryckmans' erroneous reading which becomes "Wazeba" (7). Here again, the copy ʿInân 58bis clearly shows ʿdbh (11).

(the following notes are on p.30)

(4) Cf. BASOR, No. 145, February, 1957, p. 30.
(7) Cf. Warfare in Ancient South Arabia (2nd.-3rd. Centuries A.D.), London, 1976, p. 36. For this publication. cf. JaCIASA, p. 264.
(9) Cf. Le Muséon, 69 (1956), pp. 142, 145 and 161.
(10) Cf l.c., pp. 148 and 150.
(11) Cf. above, note 8, pp. 360, l. 13, and 363, last line."

And most importantly, Nebes hides from the reader the mention of pl.5 (cf. below) which clearly shows for anyone honest enough to tell the truth - but Nebes apparently is not one of them - that the circle ʿ, ⊙, looks entirely different from the rectangular-shaped ellipsis of w, ⊡, and 3 specimens of each are close to each other.

Partial reproduction of the photo of JaMAR, X (1979), pl. 5, of the center of Ja 576/ 8-13, showing 5 ʿ's and 13 w's to the effect that there can be no possibility of misreading ʿ instead of w or vice versa. Of particular importance are ll. 8 and 9 with
1 case of ʿw, and
2 cases of w/ʿ and w/w, which
allow the best comparability possible.

A panoramic view of the situation may be presented as follows.

- My reading of ʿdbh as certain in 1951 (in my field book + photos in black and color), 1957, 1962 and 1979 is correct with no palaver of any kind possible.
- G. Ryckmans' reading of "wḏbh" as certain from a photo in 1956 is manifestly wrong.
- J. Ryckmans' reading of "wḏbh" as certain in 1957 is manifestly wrong and stubbornly partisan.
- A.F.L. Beeston's reading of "wḏbh" in 1976 is manifestly erroneous and a proof of his stubbornly partisan obscurantism.
- W.W. Müller's mention of the equally possible reading of "wḏbh" in 1978 is manifestly erroneous and a proof of his stubbornly partisan obscurantism.
- N. Nebes' 1995 reference to 1978 W.W. Müller theory on the equally possible reading of "wḏbh" is contemptible: it is an act of vile sycophancy. His failure of referring to my Miscellanées..., X (1979), pp. 29-30 and the photo of pl.5 manifests beyond any possible doubt that his main purpose in dealing with South-arabian is but a sha-

meful, servile toadyism acceptable in politics, but inadmissible in decent societies and scholarship. Summarily, a consistent pattern has already emerged from the preceding pages. Like his 2 beacons and especially A.P.L. Beeston, Nebes may be a theorist prone to classifying and organising hypothetical features based on grammatical dissections in the same manner, but much less brilliantly, as the giant of the art of scaffolding, J. Pirenne. She also started in earnest with a book printed by an academy; she initiated many a whirlwind, each of which was like an explosion of new beginnings, of new openings, and of new perspectives. But, when somebody wanted to go down to a minute analysis, he normally found a lot of empty statements, of lame arguments and, unfortunately, of wrong theories; viz a magnificent illustration of the students' axiom mentioned above, p. 95. It is the same impression that I had whilst probing many places of Nebes' booklet because, contrary to the mention of "epigraphische Untersuchungen" in the title of the booklet, Nebes is not an epigrapher. On the one hand, he manifestly lacks the inescapably necessary field experience so vividly lacking in the writings of theoreticians, such as A.P.L. Beeston, J. Pirenne, W.W. Müller and C. Robin. On the other hand, he is not, as a true epigrapher should be, a researcher imbued with deep respect for the work of any ancient writer, professional engraver or not, which is untouchable and unalterable unless an obvious error, which is never to be presumed, was committed; f.i., the case of ᶜdbh that W.W. Müller wanted to correct to "wdbh" in order to indulge his haughty pruritus of showing off. In spite of his remarks, Nebes actually does not care whether a reading is justified or not provided he flatters his beacon(s); otherwise, he would not repeat erroneous readings.

After the last decades of cerebral brainwashing by J. Pirenne on palaeography, by A.P.L. Beeston, M.A. al-Ghul and W.W. Müller on pan-classical Arabism to be imposed everywhere, the reader has to face the onslaught of historical syntheses (also based on pan-classical Arabism background) and now on syntaxic scaffoldings by N. Nebes, another systematizer in the spirit of both Beeston and Pirenne. The reader has nothing else to do but to stand firm, unwavering and wait for the end of the present-day blitzkrieg... because to pass away, it will inevitably: even the mightiest hurricane, or the most spectacular earthquake, or the most terrifying drought, or the most savage war always ends up in the ignominious nothingness of a short name followed by the year and also by a brief record of the havoc and loss of life which remains forever the ignominious signature of a fleeting prank.

January 11, 1996

Addendum to p. 210, bottom.

e = RÉS 2976 = A 197 b+a = Gl 1158+1157; A 197b shows only the 1st 2 signs of each line. This text exemplifies the case of an unspecified number of persons being the subjects of 2 singular verbs which concord with the 1st person, the man in authority, as explained above, p. 209, ll. 1-3.

Robin fails to utilise the present text apparently because he endorses RÉS's opinion, in V (1989), p. 275. The text was checked on the 2 photos of A 197.

rtd²l/bn/wdd²l/dbs	1 Ratad²il, son of Waddad²il, he of Baz-
r/shfn/kbr/²dm/mlk/m	2 ar Sahfan, leader of the servants of the king of Ma-
ᶜn/w²dm/mlk/mᶜn/s	3 ᶜin and the servants of the king of Maᶜin, has dedi-
l²/wbny/mhfdn/ysbm	4 cated and built the tower Yasbum.

G. Ryckmans' commentary is limited to the reference of what he calls "la répétition de la formule" to F. Hommel, Aufsätze und Abhandlungen, Munich, II (1909) 1910, p.232, fn 2, and also to M. Hartmann, Die Arabische Frage, Berlin, 1900-1910 = Amsterdam, 1976, "p. 610" (to be corrected to 608), fn 1, where Hommel's understanding is explained, viz

"w²dm/mlk/mᶜn" (1.3) : "es ist Bestandteil des Titels; ... da handelt es sich um König und Stamm als legislative Faktoren."

Hommel's starting point, which is not actually acknowledged by Hartmann is "... König von Maᶜân und (sein Volk) Maᶜân" - this corresponds to mlk/mᶜm/wmᶜm - which is inspired by mlk/mᶜm/wb/śᶜbs/mᶜm of RÉS 2775/2, or

mlk/mᶜm/wmśwd/mᶜm of RÉS 3458/2.

In turn, this last quotation undoubtedly influenced Hommel's last phrase quoted above as well as Hartmann's mention of "legislative Faktoren."

Unfortunately, the text has kbr/²dm/mlk/mᶜn immediately followed by

w²dm/mlk/mᶜn

where Hommel-Hartmann's explanation is inapplicable because the mention of the kbr's title is immediately followed by the collective mention of the kbr's dependents.

Finally and here again, the singular verb śl² concords with rtd²l, the man in authority speaking for himself in his title as well as on behalf of all his dependents.

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